AMERICAN PHYSICAL ANTHROPOLOGY:
A HISTORICAL PERSPECTIVE

by

Lawrence G. Quade
B.A., University of Texas at Austin, 1967

Submitted to the Department of Anthropology
and the Faculty of the Graduate School
of the University of Kansas
in partial fulfillment of the requirements
for the degree of Doctor of Philosophy.

October, 1971

Dissertation Committee:

Redacted Signature

Chairman

Redacted Signature

Redacted Signature

ACKNOWLEDGEMENTS

A number of professors, associates and friends have been instrumental in the accomplishment of this project. Foremost among them is Dr. Thomas W. McKern, who has directed my graduate studies and served as chairman of the dissertation committee. For providing the academic and professional environment which has yielded a stimulating and meaningful education, for his insistence upon highest standards, for his unfailing support and his devotion to the completion of this project, he deserves far more gratitude than I can possibly express here.

An outstanding debt is also due my dear friend and associate, Mrs. Sharon McKern. It was she who introduced me to anthropology some years ago, and through good times and bad has been a stable and stimulating influence in my educational career. Her high literary and professional standards have seen this manuscript through successive revisions. Thus, while I alone remain responsible, she is to be thanked for her efforts.

I acknowledge with gratitude the support and assistance of other members of the Department of Anthropology who have served on my dissertation committee. They are Drs. Alfred E. Johnson, Ellis R. Kerley, and William M. Bass, III. And I

thank Dr. Murray Wax of the Department of Sociology for his critical reading of the manuscript and for his helpful suggestions.

For their encouragement and support during our days as graduate students, my sincerest gratitude is due Drs. Mary Jo Kasselman, William S. Lyon, III, and D. Gentry Steele. Bill Lyon and I spent many an impromptu evening, freely and spontaneously probing many of the ideas which have been developed here. Thus in many subtle ways his influence is felt.

Finally I acknowledge with deepest gratitude a debt of a more personal nature. Glyn and Ethel Quade, my parents, have maintained throughout my lifetime a rich and open road to education--providing the stimulation, the environment, and the means, yet wisely leaving the choices to me. The present accomplishment, then, is a culmination of their efforts also. My heartfelt thanks.

L.G.Q.

Topeka, Kansas

April 1971

CONTENTS

CHAPTER I

PHYSICAL ANTHROPOLOGY IN AMERICA

Is there an American physical anthropology?

Certainly the science exists in Europe. Derived from imaginative nineteenth-century investigations into the extent of human variation and the limits of man's history on earth, European physical anthropology remains a valid field of scientific inquiry, its place among the biological sciences unquestioned. The science, called "anthropology" in Europe, is a field of inquiry distinct from prehistory, ethnography or linguistics.

But in the United States, where historical accident and long geographical isolation helped to shape the direction of early investigations, physical anthropology assumes a character distinct from its European counterpart. Structured here within the academic framework of the social sciences, it is nevertheless bound to the study of man as a biological entity--though one endowed with the capacity for culture. The biological-social duality of man is the enigma to which American physical anthropology has addressed itself. It is as a function of this orientation that it endures its disadvantages and enjoys its advantages.

How is American physical anthropology unique? How does it relate to European studies? In what way is it a part of the more general and encompassing science of man, called "anthropology" on the American continent? What problems are specific to American physical anthropology? What are its purposes and goals? What is its place in American academic structure? What is its significance and relevance outside of academia? These and other evaluative questions form the subject of this dissertation. The problem is to determine whether there is, in fact, an American physical anthropology.

What has come to be called physical anthropology on the American continent is somewhat a hybrid discipline. It emerged during a time when, in a fairly young nation, scientific pursuits were becoming firmly established. Its going was not easy because rather than following in imitative fashion its European counterpart, its American advocates and practitioners formulated their own problems and addressed themselves to specific needs within the American context.

For example, Franz Boas--one of the greatest early American anthropologists--was disturbed about widely circulated and generally accepted notions on race. Common assumption around the turn of the century said that intermixture led to racial degeneration. This belief, in turn, provided a quasi-scientific rationale for justifying racial discrimination.

In a move characteristic of the social motivation behind early American physical anthropology, Boas concentrated his efforts toward refuting such notions on race.

The historical context of the emerging science included the American Indian--who at that time was a curiosity to the whole world. The investigation of these people--both in terms of past skeletal and cultural remains, and living representatives--was a logical enterprise for American physical anthropologists. That the scientists who were studying them were in large part European-educated does not detract from the distinctively American character of their researches. An American way of doing things was developing. We are dealing with historical accident in the sense that the American Indians were discovered by the Old World at a given time and place. In this sense it is also geographical accident. Thus we have noted that among the factors contributing to the growth of the American branch of the science were race, social motivation, and historical and geographical accident. There undoubtedly were other factors.

The initial avenues of anthropological research in the New World decisively oriented the future of American physical anthropology. For although the science sought to understand man's physical nature, both past and present, the characaer of its development placed it in a social context. The result

is that while physical anthropology is, by definition, a biological science, in American academic structure it is categorically found in the framework of the social sciences. From its unique position physical anthropology has participated in symbiotic relationships with biology, human anatomy and physiology, on the one hand, and with cultural, social and linguistic anthropology and archaeology, on the other. It has drawn strength and relevance from other disciplines and has, in turn, contributed to their functions. This has been both an advantageous and a disadvantageous situation. It has provided physical anthropology with research topics, additional relevant data, interpretational framework, and an integrating overall picture. Yet it has demanded attention in its own right, and has drained physical anthropology of time and effort which might otherwise have been spent in the more direct furthering of the discipline. The situation, however, is not to be lamented. For physical anthropology in America is what it is. This is what we must set about analyzing.

The boundaries of physical anthropology are not easily defined. And simply listing research areas does not necessarily describe the true nature of the discipline. Orientations, opinions and personal interpretations are as significant in defining a discipline as are broad areas of investigation. First, physical anthropology should be viewed in the broader

anthropological context. That is, physical anthropology is but one aspect of a more general study of man. Anthropology, a word meaning literally "the study of man," denotes the whole range of investigations which relate to man. This particular feature--the feasibility of investigating any factor even vaguely relating to man--has been both an advantage and a disadvantage to anthropologists. It means that an anthropologist may choose to study modern day cultures which remain in a technologically unadvanced state. Such a study may focus upon kinship, religion, economy, the family, and a variety of other topics. Or the anthropologist may study languages and their distribution; linguistics may investigate the significance of languages as they are and function today, or it may seek to reconstruct past distributions and contacts between peoples. Anthropologists may also study the technological remains of prehistoric peoples, including both their stone and ceramic productions and other remaining indications of how and where prehistoric peoples lived. Or finally, the anthropologist may investigate the evolutionary history of man--his past and present physical and physiological nature. Such research carries the anthropologist also into non-human primate studies, in an attempt to define the biological man.

It is the fourth type of anthropological investigation which is defined as physical anthropology. It must be

remembered at this point, however, that no facet of man, be it physical, social, linguistic or archaeological, can ever be completely and accurately analyzed apart from the whole. Context is an essential element of anthropological study. The physical evolution of man, for example--and we are speaking on the broadest of scales--cannot be fully understood without consideration of the cultural remains which correspond to the various stages of evolutionary development. Or again, the study of paleopathology--the disease record among later prehistoric peoples--is illuminated also by the archaeological record and, conceivably, by information which linguistics may provide. Ethnographic analogy may also shed light on the study of paleopathology.

What we are saying, on the grander scale, is that anthropology is an integrated and unified way of thinking. Anthropology refers to the study of one thing: man. When broken into its branches, therefore, anthropology is the study of facets of a whole. In order to understand the whole, and to reach this understanding more easily, the parts are studied exclusively. And too often, anthropologists fail to remember that it is the whole which really matters. Physical anthropology, that is, is important as a sub-discipline in that it solves minute problems and sheds light on man--man as an entity.

Physical anthropology, then, emerges in a peculiar light. It is a bridge. Embracing the physical nature of man, and relating this information to the social setting in which man exists, physical anthropology assumes a dual character. It must deal with both the physical and the social. And the reason for this is the very nature of man himself. Man's zoological man, Homo sapiens, indicates that man is a "knowing," "knowledgeable," or "sapient" animal. He is the only such animal. At any given point in time and space, a specimen of Homo sapiens is both a functioning biological organism and a rational, thinking, emotive being. This, by definition. The physical anthropologist entering at this point has a grave responsibility. He must preserve the integrity of the whole--man--while manipulating its various aspects. In its own respect, each branch of anthropology demands this responsibility.

Physical anthropology has been variously defined by different authors. Hulse says that "Physical anthropology attempts to explain the biological background and the biological aspects of mankind" (1963:vii). He does not stop at this point, however. Noting that physical anthropology is more than just human biology, evolution or genetics, and that it is impossible to chop mankind into various segments, he stresses the inseparable nature of both the social and biological facts relating to man (1963:10-11). "Biology," he says, "is not the

study of the anatomy of dead creatures, but of the activity of living ones" (1963:11). It is this live, vital force which is so important to anthropology in general, and to physical anthropology in particular. Anthropology--even paleoanthropology--is not a science of the dead. It is a science of the living! If it studies past peoples, then it is the study of those who lived before, not of those who are dead now. This seemingly insignificant conceptual feature is of utmost importance in the anthropological way of thinking.

Juan Comas, in his Manual of Physical Anthropology, notes that on the American continent, where anthropology has come to mean the study of man in the broadest sense, the term now embraces several independent disciplines, rather than subdisciplines. He suggests several brief definitions--the "science which studies human variations," "comparative study of the human body and its inseparable functions," "exposition of the causes and courses of human evolution, transmission and classification, effects and tendencies in the functional and organic differences" (1960:28). Recalling Sergi's international survey (1947), in which he solicited definition of the terms "anthropology" and "ethnology" and received a heterogeneity of opinions, Comas suggests that the anthropological field of action and the growing need for association with other sciences has contributed to confusion in

anthropological definition. It seems certain, also, that as the discipline progresses, as it increases in complexity and heterogeneity, a comprehensive and workable definition of anthropology and of its constituent disciplines will become even more elusive.

It is well to remember that what we are dealing with is a semantic problem. Everyone knows--or thinks he knows--what physical anthropology is. The putting into words seems to cause the problem. But perhaps verbalizing only focuses attention on the problem--the problem being the effects of heterogeneity. Comas notes, for example, that the boundaries of physical anthropology have occasionally been confused with human biology, anatomy and physiology (1960:28-29). The boundaries certainly overlap, and inquiries of one or another nature may as well be carried out under the auspices of one formal discipline as another. The overall orientation of disciplines, however, is the differentiating factor. The most encompassing and general definitions figure here. For human biology, anatomy and physiology focus upon the structure and function of the "contemporary <u>average man</u>," while physical anthropology goes farther in its searches. Physical anthropology ingests factors relating to chronology, race and society, in its attempts to understand man in general. Anthropology, that is, studies a whole, whereas the other disciplines mentioned study parts of that whole.

Thomas W. and Sharon McKern, in Human Origins, define physical anthropology in basic terms: "the study of the development and present nature of man's physical structure" (1969a:5). Man, they say, is first investigated as a biological entity, but "always, however, with a view of his cultural capacities." Physical anthropology, then, is a part of general anthropology because of the dual nature of man. That is, it investigates the biological nature of man, but always with regard to man's capacity for culture. This particular orientation is peculiar to physical anthropology, and characterizes no other discipline. And the discipline, they continue, as does no other science "attempts a true synthesis of human biology." While physical anthropology depends in great part upon borrowing information and techniques from other disciplines, its distinctive nature lies in synthesizing. "This is physical anthropology."

This study is a critical analysis of American physical anthropology. It emerged out of a study of the history of the discipline, and it remains--by nature and by methodology--a history. That an established academic discipline has a history is a readily deducible idea. A group of people who conceive of themselves as united in a sphere of academic endeavors have arrived at the present by means of a number

of past events. The past events are structured through time and have formulated the present situation by virtue of their individual natures. Taken as a whole, these events are the history of the discipline. They are concrete building blocks of a critical, evaluative, or apologetic work.

History, it is important to note, does not exist in the abstract. W. H. Walsh says that history, being a generic term, is real only in its species. "To ask what general propositions history as such presupposes is thus to ask a question which it is unprofitable to pursue because it cannot be answered" (1960:64). Specifically, then, a number of events have transpired, with the result that a discipline or sub-discipline, called physical anthropology, has come into being. We may, therefore, study the history of physical anthropology.

Relative to the status of other academic disciplines, physical anthropology is young, even though it pre-dates general anthropology. For if academic disciplines may be thought of as progressing through a number of evolutionary stages, then physical anthropology is only now emerging from the basic stages of development. It will be one purpose of this paper to delineate and analyze the stages of development of physical anthropology. A search of the literature has shown that few attempts have been made toward telling the history of the discipline. There certainly is no single

work, nor even a composite work, which is acceptable as an up-to-date history of physical anthropology. Rather there are scattered attempts, through time and through the subject range of physical anthropology, at telling what has actually happened in physical anthropology and to those people who call themselves physical anthropologists. Introductory texts and general works on physical anthropology provide historical sketches, but these vary according to the orientations and purposes of the writers; they are not comprehensive.

The void is both literary and scientific. In recognition of the void it should be valuable to look into the past of physical anthropology. This dissertation is a story of physical anthropology in the United States. It is not the only such story, but it is unique in its orientation. It is a history. But it is neither a complete history nor completely historical. It is a record of how physical anthropology came to be in the New World. Yet it seeks to answer more than merely "how." It approaches questions such as "why?" "with what results?" and "to what effect?" It examines the discipline within the context of the academic structure, in an attempt to evaluate the standing of the discipline and to justify its inclusion therein.

This study will attempt to go beyond mere events, however, to the causes and effects of discrete temporal and spatial

events. No event, whether it be a committee decision, research project, publication, seminar, or conversation, exists or has existed in a vacuum. Rather, it is a part of the total milieu of events--of thought and being--which form the progressive present. An event may be isolated, handled and scrutinized, squeezed for its meaning. And such a minute study of an event is an avenue by which the event may be understood and known in detail. Out of context, however, the event is nothing more than an isolated happening. An occurrence in time and space, with little meaning beyond itself. There is a way to reach meaning, however, and that is context. Put back into its context, a discrete event assumes its full body of meaning and importance. It becomes a living occurrence which was the culmination of all preceeding events, and which influenced events subsequent to its happening. That is, the event becomes an integral part of history.

It is this process of conveying meaning, of showing the integrated picture, which distinguishes what Walsh calls "significant" history from "plain" history. Whenever the historian confines himself to a narration of events and to the reconstruction of a chronology of events, he is creating a "plain" history. If he makes attempts at explaining that narration or chronology, however, he constructs a "significant" history. Although both types serve a purpose, it is the latter

sort of history which most historians seek to create. Walsh explains that

> The historian is not content to tell us merely what happened; he wishes to make us see why it happened, too. In other words, he aims... at a reconstruction of the past which is both intelligent and intelligible. It is true that historians often fail to reach this high level: they lack either the evidence or the insight required for an adequate reconstruction, and find themselves in consequence driven to recite isolated facts without being able to fit them into a coherent picture. But their doing so testifies only to the general difficulties under which historians work, not to any inherent weakness in the historical ideal. The truth is that history is a much more difficult subject than it is often taken to be, and that its successful pursuit demands the fulfillment of many conditions, not all of which are in the power of historians themselves (1960:32).

Ways of knowing and ways of understanding are problems in the reconstruction of a history. For while discrete events may be isolated and scrutinized and explained--while accuracy and objectivity may be attained from pertinent written accounts--analysis of the broader meaning of an event is fraught with subjectivity. Even more, the understanding of the broader history is subject to individual whim, to individual analysis, to personal diagnosis and assessment. The implication made here is that subjectivity is undesirable, objectivity to be sought. For the most part such is the case. A history is the personal work of the historian, or it is nothing more than a rote chronology. Chronology is not the

present goal. Rather, understanding. Understanding and assessment, however, do not preclude objectivity; they only place it farther from attainment. A relatively true history, then, is the present goal. A recapitulation of events, clothed in their context with meaning and understanding, objectified but also subject to occasional personal interpretation. This is the history which the present attempt has sought. The history should be judged within this framework.

In many respects the present work resembles an apology. Apology, in common usage, is an acknowledgement, reparation, or expression of regret for some act which was improper, discourteous or injurious. As used philosophically, however, the word is seldom seen nowadays. Apologies are seldom read, and less frequently written. In the classic sense, apology was a work written in defense or justification of something which had been accused of being wrong or unjustifiable. John Jewel's An Apology of the Church of England, written in the 1500's, is a case in point.

The present work differs from a classic apology in that it does not come in the midst of a flurry of accusations or denunciations. It is not written in a fever of defense, the accomplishment of which will provide the immediate self-justification of a number of people. Rather, this is

essentially an explanation, definition, and justification for being. Not all needs for an apology, after all, are as pressing as was the Anglican Church's need in the sixteenth century. The explanation and definition of a body may be on a much commoner plane, less related to the needs of the nation at large. And this need not detract from the basic importance of an apologetic work. Among the purposes of this study, then, is the demonstration that the concept retains some utility in modern academia, that it can enable us to understand in greater depth the context and character of scholarly pursuits.

There once was a time, in American physical anthropology, when an apology was critically needed. The discipline--actually only a group of scientists and naturalists bound by a common orientation toward the study of man--was very young. It was floundering and struggling among the established powers of anatomy, medicine, biology and others. Anthropology, as it was called at that time, was looked on with disdain by many scientists. Searching for respectability, the discipline faced problems in the recruitment of personnel, procuring of financial support, securing of public interest and support, acceptance in academic structure (which was considerably more rigid than at present), and other problems. During this critical period, Ales Hrdlicka, then Curator of

Physical Anthropology at the U.S. National Museum, summarized the problems facing the discipline, while outlining the possible avenues for overcoming difficulties and attaining respectability. This work is the well-known Physical Anthropology. Its scope and aims; its history and present status in the United States (1919b). Although Hrdlicka did not so name it, this monograph was, in scope and orientation, an apology. The tone of discussion, the choices of words and ideas, all show that Hrdlicka had in mind a justification of his discipline to the rest of the world.

Hrdlicka's monograph served its purpose at the time, and it serves as a particularly lucid history today. But it did not, as a sole force, exonerate American physical anthropology. Whether American physical anthropology has been exonerated and whether it has a legitimate place in American academic structure, is the question herein addressed. For while physical anthropologists rarely are accused as illegitimate space-takers--that is, they are accepted today--there is good reason to look behind and through the present into how they came to be where they are. Understanding of the difficulties of the past can aid in understanding the deficiencies of today. Understanding the pressures which were brought to bear yesterday, as well as the entire context out of which the discipline emerged, may shed light on the significance of the

whole discipline as it exists and functions today. Among American physical anthropologists, for example, why do so many primatologists do their observations and researches in laboratories? What particular problems do paleoanthropologists face? How has race figured in the development of anthropology and what is its relevance today? How have the scope and importance of osteological studies changed? These questions and others like them are most easily and clearly understood through illumination by the past.

Because these questions, when increased a hundredfold, embrace the emergence of American physical anthropology, the discussion and answering of the questions may, taken as a whole, be much more than a history or recapitulation. The discussion may serve as an apology, for this is what it accomplishes. Or it may result in establishing a particular discipline called American physical anthropology.

The idea of questioning and justifying existence is a repugnant idea to some learned people. To some it smacks of fruitless endeavor, flagrant waste of time, and ostentatious pedantism. Perhaps in the past philosophical self-examination has proven a bit distasteful to scientists, in as much as such questions led them afield from their specializations, and into philosophical questions. Perhaps also, such endeavors have in the past been worthless endeavors. This need not be

the case, however. For self-examination can lead to a greater and deeper understanding of matters which we say we understand, but which we actually understand only in part or not at all. Total immersion in a discipline does not necessarily indicate a comparable depth of understanding, thereby obviating self-analytic thought. On the contrary, one may find that such intimate contact with his subject tends to obscure certain of its elements. This work, then, seeks to answer questions. It is directed toward those who seek to know more of the rationale behind anthropology. It is with such matters at the forefront, that this work is approached. If the reader enters with a corresponding openness, he should find that the work is more than merely a mental exercise.

There are other reasons why an analysis and justification of their discipline should be of value to physical anthropologists. Not the least of these is the possibility that American physical anthropology does not validly exist as an academic discipline. It was just stated that we are generally accepted now. But is existence by virtue of common consent and acceptance sufficiently explained? I think not, if we are to go beyond immediate concerns of a budgetary and administrative nature. We now have budgets and research funds (although, as we shall see, the latter rests on precarious grounds). But if we are to concern ourselves with more

ultimate meaning, on a higher plane of thought, then an apologetic work is surely in order. For within the literature of (and about, for that matter) physical anthropology, nowhere have I encountered a sound rationalization for the aims of the discipline as a whole. Hrdlicka's monograph (1919b) probably approximates most closely such a work. Half a century old, however, it does not take into account recent and modern functions of the discipline and its adherents and hence is outdated.

Now while no adequate defense has been presented for the discipline as a whole, individual topics of research customarily are explained, rationalized, and consequently justified. Hence the claim may be advanced that physical anthropology's existence has been justified through a series of calculated steps--that is, by virtue of the literature which exists, the individual parts of which have been separately and individually justified. This brings us to another consideration, however. The latter method provides for no overall integration. Physical anthropology is a complex discipline, the more complicated in that it actually constitutes a sub-discipline within the broader study of anthropology. The history of the discipline embraces the origin and development of such constituent endeavors as the studies of race, primatology, fossil man, osteology, paleoanthropology, growth and

development, human anatomy, and so on. Can such diversified study be treated philosophically as a unified academic discipline? Can a single apologetic work actually embrace, in its scope, the whole of the domain of physical anthropology? Washburn has spoken of the conglomerate, and his concept seems to hold a great deal of value for our study. For there seems to be a corporate body of physical anthropological thought, whether it is succinctly set down or not. A distinct group of people uniformly think of themselves as physical anthropologists, despite their complementary titles, such as osteologist or anatomist. These are evidences of unification.

Further support for the placement of physical anthropology in academic structure is found in the argument that no two disciplines are really alike. Thus, each must be justified on its own grounds. Berger (1963), for example, says that sociology is distinctively different from any other discipline, in terms of its methods, goals and procedures. Perhaps this is true for all disciplines, however. For while certain rules require conformation, and criteria must be satisfied, each discipline has considerable latitude in this respect. It is one purpose of this work to define such boundaries.

In order to define boundaries, facilitate tracing the history of American physical anthropology, and construct the apologetic framework for justifying the American branch of

physical anthropology, several criteria or prerequisites have been investigated. The criteria relate to the past, present and future of academic disciplines in general, providing, in effect, analyses of the several aspects of a discipline. Hopefully, the criteria may be taken as a whole, with view toward assessing the "academic standing" of the discipline in question. The prerequisites--so called in the sense that they are conditions which must be satisfied in order to validate the position of a discipline within academic structure--were not devised solely with physical anthropology in mind. Rather they should be prerequisites useful to self-examination with other disciplines.

Prerequisites were not selected at random, but rather through a process of investigation, including addition, elimination and conjugation. It is felt that the six criteria which have emerged encompass the functional and theoretical boundaries of any given discipline. The prerequisites, each of which will be investigated in turn, are as follows:

1. There must be a group of people who conceive of themselves as unified in their scholarly attempts, as belonging to a discipline.

2. A chronology of events and thought should exist--a history. The first two prerequisites are almost axiomatic, in as much as there would be no question of a discipline's existence were the two criteria not fulfilled. This is not to diminish their importance,

however, since the successful construction of an apologetic work depends in great part upon people and history.

3. There must be a theoretical framework within the discipline, for the incorporation of goals, methods, procedures, and so forth. A basic theoretical structure is needed, within which all research and study may fit. In this respect, an apology of the discipline is also appropriate.

4. A domain of specimens and physical data should be available for the especial use of practitioners of a discipline. It is this prerequisite which is of critical importance to the present investigation.

5. A valid discipline will have an accumulated body of raw data, original research, and other relevant studies, constituting the peculiar orientation of the discipline. This criterion differs from the fourth in that it deals with ideas, theory and works, rather than things. The fifth criterion, then, is the conceptual and analytic framework for handling the physical specimens of criterion four.

6. A discipline which is properly placed in academia will have prospects for future growth and development, and relevance to the future.

CHAPTER II

PHYSICAL ANTHROPOLOGISTS

The first prerequisite states that in order for a discipline to be properly accorded status in academic structure, it must be comprised of a group of people who are in some fashion unified. Unity, unqualified, is not the sole factor however. For there is an inherent notion of exclusiveness. That is, not only must a unity be mutually recognized and felt, but a group of scholars should conceive of themselves as committed to a common endeavor, related in a specific scholarly enterprise.

Students of the biological nature of man have called themselves physical anthropologists since the latter decades of the nineteenth century (Hrdlicka 1919b:7-8). The name came into common usage to designate those scholars who studied the origin, nature and varieties of Homo sapiens. We have already seen that the corresponding group of people on the European front go by the name "anthropologists," and that it is only the Americans who must distinguish their discipline with the adjective "physical." The American branch originated, in many respects, not out of the European branch, in evolutionary fashion, but rather as a splinter group of American anatomists and physicians who inaugurated a new orientation

toward anatomical and prehistoric questions (see Chapter III). The group was unified in general by Hrdlicka's scientific and administrative efforts. It seems safe to say that Hrdlicka was the primary cohesive force in binding the systematic and administrative aspects of a struggling new science.

As time went on, scholars of greater variety were inducted into physical anthropology. Among the more enduring names, for example, we note that Hooton was educated in the classics, and gradually shifted his interests to the physical history and biology of man. Boas was a physicist who became engaged in geophysical interests, pursued geographical studies, and eventually devoted himself to a driving interest in the American Indian, thereby becoming one of the most important early American anthropologists. Hrdlicka was a homeopath who redirected his interests and proceeded to advance to the forefront of academics. Increasingly also, comparative anatomists and vertebrate paleontologists contributed to anthropological endeavors. As a consequence of such heterogeneity, the nature of research topics became more diversified, so that whereas the first American physical anthropologists were essentially bound by common techniques and orientations, soon there was a whole new field of scientific endeavor. Techniques and a way of thinking gave way to a more encompassing field of inquiry. These developments are discussed in greater detail

in the next chapter. The essential point here is that those who called themselves physical anthropologists soon had far greater latitude in choosing among a generally acceptable range of research topics, with the result that the physical anthropologist of today is usually defined in terms of his interests in human biology (Lasker 1964:3) or in the processes of primate evolution and human variation (Washburn 1951, 1953). In any case, human variation is the central theme for those who call themselves physical anthropologists.

Furthermore, it is significant that while physical anthropologists have experienced change in scope and direction during the past century, they concurrently have built and maintained a unifying identity. They have continuously thought of themselves as constituting one body--physical anthropologists. As the scope of the discipline has evolved, eager and interested scholars and various research areas have been incorporated into the discipline or, in some cases, phased out of it. Yet the common core survives. If this characteristic is demonstrable in the functionings of all extant academic disciplines, it is no less true for physical anthropology in America.

It was stated in the introduction that the primary purpose of this dissertation is to determine whether there is, in fact, an American physical anthropology, as distinct from

the European counterpart. This question is illuminated by examining the way in which the respective scientists spend their time and efforts. Inquiry into the nature of anthropology departments in the United States, both past and present, reveals that the primary academic interest is in teaching. The majority of American students who become anthropologists eventually become teachers, concurrently conducting research on a less extensive scale. This state of affairs is not generally characteristic of European anthropology. Specialized training--corresponding to our doctoral programs--is more exclusive there. It is reserved for the qualified few rather than opened to any interested student. Hence, European anthropology retains a more academically reserved character, partially removed from everyday teaching affairs. Pure research--apart from practical application--for example, is more characteristic of European than of American endeavors. Americans have a passion for justifying and for demonstrating utility and relevance of whatever they do. This difference in outlook--a cultural feature resultant of historical accident--helps to explain why Europeans are more research-oriented while Americans direct their attention toward teaching.

The American predilection for communicating ideas to the public--for teaching--is intricately enmeshed with the American educational system and American values at large, so that

it is no longer possible to say which factor is causally related to the other. They exist together. The American goal that everyone shall become as educated as is feasible, with increasing attention to the younger segments of the population, has had increasing effect upon even so removed and specialized a field as physical anthropology. Almost all the monetary support which physical anthropologists have mustered flows through the educational system. Accordingly, physical anthropologists have found themselves--with other academicians and scientists--encouraged, even compelled, to teach the general university student population. The very academic structure is such, in most cases, that a professional is hired to teach. Processes of job-seeking and of employment are couched in terms of classroom instruction.

In order to present a more balanced picture, however, it should be noted that there are exceptions to the American tendency. Some museum positions, curatorial and research-oriented, are open to physical anthropologists. Likewise, some university departments employ scientists for the purpose of research, with a minimum teaching load. The latter condition, however, is reserved almost exclusively for distinguished scholars; such positions are seldom open to younger, less experienced professionals. And while teaching remains the prime consideration, allowances are made, in most larger

state-supported educational institutions, for research. Facilities and time are provided, and physical anthropologists divide their time between teaching and research.

In addition, physical anthropologists are sometimes employed outside of academia and museums. Beginning with Hooton's first applications of physical anthropology--correlating body measurements with the dimensions of public seating facilities--anthropologists have become increasingly involved with problems of industrial design. It is surprising, in fact, that physical anthropologists were so tardy in developing the practical applications of their science, to the benefit of the general public. This is especially true in light of the American penchant for being relevant and for justifying existence. Since this time, however, American physical anthropologists have indeed become involved in human problems. The technological society in which they function, and which provides their financial support, demands that they produce readily usable information and direct some of their efforts toward solving problems which the technological society-at-large faces.

The field of applied physical anthropology has come to be known in some quarters as "human engineering." Automotive and public transportation manufacturers consult and employ human engineers in order to better design the equipment which

must accomodate the human body. Furniture manufacturers use anthropometric data in a similar fashion. Likewise, clothiers consult anthropological data in order to determine percentages of body-size distribution, that they may thereby calculate the number of needed garments in any given size. The military complex and its subsidiaries have also hired physical anthropologists, because of increasing difficulties in manufacturing aircraft control compartments. Aircraft controls had become so numerous and complex, with resulting confusion in planning, that human engineers were needed in order to rearrange compartments. The result has been machinery which better accomodates the human form, in its anatomical and functional capacities. H. T. E. Hertzberg is notable for his work in aircraft planning.

Practical applications of physical anthropology are also made in the area of human identification. Usually confining their attention to skeletal remains, anthropologists seek to identify victims of mass disaster (as climatic catastrophies and war), homicide and accident. McKern and McKern (1969c) have pointed out that positive identification of war dead has increased steadily, percentage-wise, since the Civil War, when only about 30 per cent of the dead were properly identified. Rising through the First and Second World Wars and the Korean War, the percentage of identification in VietNam today stands

at about 97 per cent. It has been through refinement of techniques (see Wilton M. Krogman's excellent survey of the history and methods of skeletal identification for legal purposes, 1962) and the accomplishment of key studies (e.g. McKern and Stewart 1957), in conjunction with the organizational framework of the Quartermaster Research and Development Command, that accomplishments in identifying war dead have been realized. It is curious that among the many nations of the world, only the United States consistently repatriates its war dead and seeks to identify each individual. Physical anthropologists have both assisted in accomplishing the goal and utilized the resultant data for subsequent studies.

A consequence of the American orientation toward teaching is an expanding job market. Through publicizing and popularizing their field, American physical anthropologists have increased their numbers. Thus, while even now the number of professional personnel is only several hundred, the discipline is rapidly beginning to increase in size. The implications of this situation are difficult to evaluate. From one viewpoint, American physical anthropologists may have suffered as a discipline, in as much as they have pursued research "half-time," rather than whole-heartedly and one-directedly

stimulating broad research areas. This consideration, however, is one for individual evaluation. It cannot be stated categorically that one orientation is superior to the other. It can only be stated that they are different and that they serve different purposes. It is important, however, that the distinction be clearly understood, for the situation as a whole makes for a great difference in the overall scope and orientation of the European and American disciplines, as well as in the ultimate direction and effect of the professional's endeavors.

We have seen that the pronounced heterogeneity in American physical anthropology is a direct result of the variety of people who have joined the field and who call themselves physical anthropologists. Anthropology, to begin with, has always been a melting pot. Today, for example, anthropology is widely used by American university students as an elective, as coursework to round out a general liberal arts education. All branches of anthropology are found to serve this function. As a result, a broad spectrum of students are exposed to anthropology--far more, perhaps, than might be so exposed under European academic structure. In the process of general education some of these students discover the prospects of anthropology as a profession, and undertake advanced study. Spreading the discipline through general university teaching, in

other words, has resulted in the attraction of students who might never have become interested otherwise. And American physical anthropology seems to have profited from its extensive contact with the university student population.

Still more anthropologists have reached the discipline by way of other avenues. An extremely heterogeneous group of people join the ranks for varying reasons, all of whom are satisfied in one way or another by anthropology. Among them are other scientists who are led by the desire to relate their interests and knowledge more intimately to the study of man. Some of these scientists are geologists, geographers, environmentalists, paleontologists, anatomists, physicians, biologists, zoologists, systematists, primatologists, and so on. The list becomes limitless, bounded only by the interests of man in man.

The result is that those who call themselves physical anthropologists are bound, in the final analysis, only by a common interest or orientation--and not necessarily by what they do or how they spend their professional hours. This is both an advantageous and a disadvantageous situation for the discipline itself. It is advantageous in that it broadens the scope and relevance of physical anthropology, consequently attracting even more varied students. It is disadvantageous in that integration of and standardization within the

discipline are hampered. Put in simple language, it is difficult to get so many people, with so many different orientations and beliefs, to agree on standards.

The disadvantage need not concern us unduly, however. And the heterogeneity of the discipline should not be over-emphasized. For American physical anthropology functions as a whole, and its members seldom think of themselves in such disunited or fractionated terms. It is, rather, in the analysis of the constitution of the discipline that we begin to uncover the actual variations in scholars and come to assess the significance of heterogeneity. The discipline will follow its course of development, so that we need not dwell on heterogeneity. The need now is for integration and synthesis of all these anthropologists' efforts. The time has now come for the armchair anthropologist to return in his own right. We have researched for a century. Now it is time to tie things together.

CHAPTER III

HISTORY OF THE DISCIPLINE

It has been shown in the introduction that physical anthropology was a functioning science before the actual term "anthropology" came into being. The areas of study and research which have since become the domain of the physical anthropologist were originally undertaken as peripheral or as specialized interests by physicians and anatomists. During the nineteenth century these interests were of an isolated and individualistic nature. Studies were independent of one another, dependent upon the interests of the scholar. And only gradually did a core of research begin to emerge from these isolated interests. Further, only gradually did the peculiar research orientation, which was early American physical anthropology, begin to emerge as a discipline and leave its formative stages.

Much of the early research, which will be described in this chapter, was related to the American Indian. From this newly encountered group of people came studies of race and racial differentiation, as well as cultural studies. Almost every account of westward exploration makes mention of the physical and cultural differences pertaining to the various native populations. The Lewis and Clark expedition of 1804,

for example, was charged with investigating Indian peoples--a factor which, Hrdlicka suggests (1919b:29-30), greatly stimulated general interest in the American Indian at that time. Other westward explorations during the middle years of the nineteenth century continued to awaken Anglo-Americans to the physical and cultural diversity of the peoples who first inhabited the continent. Among the government explorations were those of Wilkes (1838-42), Fremont (1842-44), Emory (1846-47), Stansbury (1849), and others. The Pacific Railroad Surveys of 1853-54 incorporated the explorations of Parke, Whipple, Pope, Stephens, Williamson and others. All of these explorations served to enlighten white Americans and stimulated scientists to step up their investigations of the American Indian. In great part, these explorations laid the foundations for the Bureau of American Ethnology--an institution of important but peripheral interest to the physical anthropologist (Hrdlicka 1919b:40-41).

The growth of physical anthropology during this early period was stimulated in great part by the scientific societies which were beginning to emerge. The American Antiquarian Society, for example, has made substantial contributions to physical anthropology, although its scope is considerably broader than this science alone. The Society has a long,

methodical history in the study of American antiquities. Its incorporation by the Massachusetts Legislature dates October 24, 1812, and the first meeting was held in Boston on November 19 of that year. The Society was founded, housed and endowed by Isaiah Thomas (1749-1831), who was its first President, from 1812 to 1831. The Society, essentially "a national library of American history," is located in Worcester, a site originally chosen in order to avoid larger cities and the wartime dangers of the coast.

Through its century and a half duration, the Society has become a primary research facility for students of American history. Not only has the library accumulated most of the reports of scientific researches on American history and prehistory since the beginning of these interests, but the Society has also sponsored publications for the scientific expression of its particular interests. The Transactions, consisting of extensive research reports, was established in 1820. Twelve volumes were published through 1911, when publication was terminated. In the Transactions are data, documents and studies, of considerable anthropological import in the early development of the discipline. The Proceedings of the Society have been published twice yearly, to form an annual volume, since 1849. Many papers on the American Indian, his history, archaeology and linguistics, have appeared in this periodical.

The membership of the American Antiquarian Society has always been something of a star-studded list. Twelve American Presidents, for example, were members. Calvin Coolidge was President of the Society at the time of his death. Numerous natural historians, anthropologists and other scientists have participated in the functionings of the Society.

Soon after the founding of the Antiquarian Society, in 1814, the Linnean Society was founded in Boston. Despite Hrdlicka's unenthusiastic attitude--"there is no evidence that the study of man derived any special stimulus through the activities of this organization" (1919b:30)--we must note that the Linnean Society was the forerunner of the Boston Society of Natural History, a society which has indeed furthered the study of man, albeit at times indirectly. The first meeting of the Society was held in 1830, in the house of Dr. Walter Channing, who was designated Chairman. Others present were Simon E. Greene, Dr. George Hayward, Dr. John Ware, Mr. Edward Brooks, Dr. Amos Binney, and Mr. George B. Emerson. Although Thomas Nuttall, the English scholar, was elected the first President of the Society, he declined office because he regarded himself a transient (Creed 1930).

Remarks by an original member of the Society impart a notion of the character of the Society in its early days, as well as the general atmosphere in which this Society and

others emerged. Because of their historical significance they are given here in their entirety. Creed, who edited the centennial volume for the Society, did not further identify the author. It may be assumed that the remarks followed by a few years the founding of the Society.

> At the time of the establishment of the Society, there was not, I believe, in New England, an institution devoted to the study of Natural History. There was not a college in New England, excepting Yale, where philosophical geology of the modern school was taught. There was not a work extant by a New England author which presumed to group the geological structure of any portion of our territory of greater extent than a county. There was not in existence a bare catalogue, to say nothing of a general history, of the animals of Massachusetts, of any class. There was not within our borders a single museum of Natural History founded according to the requirements and based upon the system of modern science, nor a single journal advocating exclusively its interests... The Laborers in Natural History worked alone without aid or encouragement from others engaged in the same pursuits, and without the approbation of the public mind, which regarded them as busy triflers (Creed 1930:4, 7).

The Society faced problems other than public disdain. Finances were discouraging for a time, until 1838, when Ambrose S. Courtis, a merchant, bequeathed a sizeable sum which subsidized the Society for some 25 years.

The Society was active in biology, zoology and paleontology. Due to the efforts of such members as Louis Agassiz and Jeffries Wyman, the Society both witnessed and effectuated changes in the general nature of natural history study.

Around 1860, Agassiz, a noted Swiss geologist who was a pioneer in the study of glaciation, began publicizing the importance of natural history in general education. He held that the study of natural phenomena was as important in developing human faculties and disciplining intellectual powers, as were other studies, and felt that the time was ripe for integrating natural history into education--a possibility which awaited only the personnel to accomplish the task. By 1861, the status of natural history, in the public mind, had risen from obscurity and derision to sympathetic interest, a feat largely due to Agassiz's efforts (Creed 1930:13-14). Wyman, who was also interested in cultivating public good will, felt that the public was actually interested in nature, and that if collections and studies were made available to the people, then their support could be expected.

Agassiz and Wyman are two names among many which are found both in the records of the Boston Society of Natural History and in the development of American physical anthropology. Agassiz was elected an honorary member of the Society in 1837, while he was still in Switzerland. Wyman served as President and was also Curator, at other intervals, of Mammals, Reptiles and Fishes, Reptiles, and Comparative Anatomy and Mammals. John C. Warren was President of the Society, as was Frederick W. Putnam, who was also a Vice President

and Curator of Ichthyology. H. P. Bowditch was also a member. The anthropological contributions of these men will be discussed later.

The Society began publishing the Boston Journal of Natural History in 1834. The first volume of its Proceedings appeared in 1844, and the first volume of the Memoirs in 1866. Still other publications have appeared under the auspices of the Boston Society of Natural History.

The Journal of the Academy of Natural Sciences of Philadelphia initiated publication in 1817. The "Introduction" in the first issue relates that although the members of the Academy had for some years been accustomed to meet at leisure hours for the purpose of communicating facts and observations, they were now determined to communicate their findings to the public. The proposal for editorial policies expressly omitted theoretical papers, suggesting that only factual material be submitted. Geological and paleontological studies were most frequently reported during the earlier years of publication, although notes on living forms also appeared.

Edward J. Nolan, in his "Introduction" to the centennial Index of the Journal and Proceedings (begun in 1841) of the Academy, reported that the Journal was begun five years after the foundation of the Academy. The Academy, then, was founded in 1812. The Act of Incorporation, however, was not proposed

and approved until 1817, at the approximate time of the launching of the Journal. The Act reads,

> ...a number of persons have formed a society in Philadelphia for the encouragement and cultivation of the sciences, by the name of "The Academy of Natural Sciences of Philadelphia," as a society devoted entirely to the advancement of useful learning... (in the Journal 1:193, 1817).

Much of the information to come out of the Academy in the early years was either directly or indirectly related to the study of man. The names of the forerunners of American physical anthropology, as well as the early students of that science, appear frequently in the publications of the Academy. It will be seen shortly that, through the influence of Samuel G. Morton, the Academy played a definitive role in the emergence of the discipline.

The role of the American Ethnological Society in the development of American physical anthropology has been formulatory, directive, supportive, and generally indispensible. The express purpose of the Society is articulated in Article II of the Constitution, which was adopted December 7, 1844:

> The objects of this Society shall comprise inquiries into the origin, progress, and characteristics of the various races of man (Transactions of the American Ethnological Society, vol. 1).

The founders of the Society, in other words, approached in a scientific and organizational fashion the specific interests which were at the same time providing for the emergence of American physical anthropology. As will be seen later, the very term "ethnology" related in the nineteenth century not to studies of culture, but rather to the more embracing study of man, with particular attention to racial variation. It seems, also, that the emergence of the various societies interested in man and his place in nature were in fact a reflection of the tenor of the times. Man was beginning to focus on himself in an entirely new way--that is, as an integral part of nature. The goal, then, was to understand man in terms of his past, his development, and his relationship to the rest of nature. The studies at this time, however, differed from those of the centuries past in that science had advanced to the point that a new understanding of these questions could be had. Man could understand, in a way previously unknown, what he was. The appearance of learned societies was, in other words, an expression of the current state and nature of mankind.

The Transactions of the American Ethnological Society began publication in 1845, the year after the founding of the Society. Statements in the Preface to the first volume (pp. ix-x) further illustrate the thinking of the times.

> The American Ethnological Society was established for the promotion of a most important and interesting branch of knowledge, that of Man and the Globe he inhabits, as comprised in the term Ethnology in its widest acceptation.
>
> The ground marked out for the operations of the Society, is unoccupied by any institution in the United States. But the establishment of similar societies in England and France, shows the general sense of the importance of ethnological investigations. These are felt to be of daily increasing moment in relation to the commercial and maritime interests of the nation, the missionary enterprise, the study of comparative philology, and many other objects of practical utility.

These statements are revealing in several respects. First, the purpose of further understanding man and his environment is obvious. We notice also the disposition of the members of this Society, as with most others at the time, to view their interests and their organization as unique. Certainly each society and academy was unique in its orientation and goals. A cursory examination of the proposals of each, however, implies that each body of men felt themselves to be pioneers, alone and unequaled in their scientific pursuits, despite the fact that many societies overlapped in their intentions. That scientists and other qualified individuals were unaware of similar activities in nearby cities is doubtful. Hence the seeming provincialism may be attributed to both lack of regular communication and a desire to provide justification for a new institution.

Finally we note in these statements the persistent concern, so American in character, for relevance and justification. The need for ethnological societies is presented in terms of "commercial and maritime interests," the "missionary enterprise," and other practical ends. It is well to remember also that these societies were arising in an environment generally disapprobate of "pure scientific endeavors" and "useless philosophic questioning." Americans of the eighteenth and nineteenth century were a hard and practical lot. Their life demanded that survival be given first consideration. Scientific societies of the period were characteristic of an initial surfacing above the constraints of a survival existence, an accomplishment which was only beginning to materialize in the new nation. As further remarks from the Preface reveal, the time was ripe for individual and leisurely prusuit of topics of interest:

> To its native and resident members, the American Ethnological Society feels it has but to indicate the field presented for their exertions, and the immense extent and variety of subjects that call for their investigation. The mystery that still envelopes the history and origin of the American races of man--the phenomena connected therewith--the diversity of languages--the remains of ancient art and traces of ancient civilization among the aborigines of Peru, Mexico, and Central America--the spontaneous growth or imported origin of arts, science, and mythology--the earthworks of the Ohio and Mississippi valleys and their founders: --these are amongst the topics for inquiry which the most cursory

> view suggests; and there are few individuals in our western country who may not obtain interesting materials for their elucidation.

In terms of sheer quantity and magnitude, no institution has made contributions to anthropology comparable to those of the Smithsonian Institution. The Institution is today by far the largest of its kind, and it was probably the first establishment in the United States to have a staff of full-time research scientists representing various fields of inquiry. The Institution owes its founding to a provision in the will of James Smithson, a noted British chemist and mineralogist. When he died in 1829, Smithson--who had never visited the United States--left $550,000 to the country for the establishment of an institution for the "increase and diffusion of knowledge among men." After considerable deliberation, Congress accepted the gift in trust, and established the Smithsonian Institution on August 10, 1846. Additional gifts have increased the basic fund to $4,500,000.

The Smithsonian, contrary to common assumption, is not a federal agency. It does, however, administer several government agencies for which Congress appropriates funds. Its overall structure is governed by the Vice President of the United States, the Chief Justice of the Supreme Court, three Senators, three Representatives, and six private citizens. The Secretary of the Institution is its Chief Executive.

The first Secretary, Joseph Henry--who was a professor at Princeton University, working on the electromagnet, before accepting the position--was instrumental in directing the Institution and its personnel. He opened great areas of the West for research--for the collection and study of the native vegetation, wildlife, and Indian inhabitants. It is apparent therefore that from its beginning the Smithsonian contributed to the study of man and his environment. Henry's administration was marked also by the establishment of international exchanges of scientific literature. Several publications of the Institution were inaugurated during his tenure as Secretary.

Spencer F. Baird, who followed Henry as Secretary, was largely responsible for developing the U.S. National Museum, a branch of the Institution, the legal provision of which was the same 1846 act of Congress. The holdings of the Museum, numbering in countless millions, rank it among the largest museums in the world. Its contributions to science in general and to anthropology in particular have earned for it an enduring and prominent position in the academic world. The Museum and the Smithsonian Institution as a whole are often thought to be the apex of organized academic endeavor.

Another branch, the Bureau of American Ethnology, was likewise authorized by the 1846 act of Congress, but was not established until 1879. Until this date anthropological and

ethnological research, especially that under the direction of John Wesley Powell and his exploration parties, was conducted in a general cooperative scientific atmosphere. In 1879, however, the various western survey parties united as the U.S. Geological Survey, and anthropological studies were transferred to the Smithsonian Institution, whereupon the Bureau was created. The Bureau carried on the studies of Indian tribes and published numerous monographs and reports. The holdings and contributions of the Smithsonian were greatly increased through the efforts of Powell and the personnel of the Bureau.

The "diffusion of knowledge" which Smithson hoped to promote has come about largely through the publication of a series of periodicals and reports on progress in the different areas of research. The Annual Reports of the Smithosnian Institution, which included for a time the Reports of the National Museum (now published separately), were begun in 1846. In addition to business matters, the Reports and Annual Reports contain brief scientific papers. The large and impressive volumes of the Smithsonian Contributions to Knowledge were published between 1848 and 1916. Major research reports formed the Contributions. The Smithsonian Miscellaneous Collections, which began publication in 1862, have survived to the present day as a primary publication outlet for research scientists,

particularly those scholars who are residents of the Smithsonian and its branches. The Bulletins of the U.S. National Museum were inaugurated in 1875, and the Proceedings of the U.S. National Museum in 1878, both continuing to the present. The Annual Reports of the Bureau of American Ethnology date from 1880, and the Bulletins of the Bureau of American Ethnology from 1886, both of which ceased when the Bureau was incorporated into the Office of Anthropology in 1965.

The practical history of the Wistar Institute of Anatomy and Biology extends back into the eighteenth century, although it was not officially organized until 1892. Caspar Wistar (1761-1818) was a natural scientist and comparative anatomist. He held a bachelor of medicine degree from the University of Pennsylvania (1782) and a medical degree from Edinburgh University (1786). He was President of the Royal Medical Society and the "Society for the further investigation of natural history." He was a member of the College of Physicians, Pennsylvania, until his death, holding the Chair of Anatomy at the University of Pennsylvania from 1808. In 1815 he followed Thomas Jefferson as President of the American Philosophical Society.

Wistar's primary contribution to anatomy was the discussion and demonstration of the posterior ethmoids and their

relation to the sphenoid bone (Packard 1942:98). His researches led him to assemble a rather substantial anatomical collection which at his death was left to the University of Pennsylvania. A similar collection assembled by William Edmonds Horner, another physician, was likewise deposited with the University. In 1892 Wistar's nephew, General Isaac Wistar, provided an endowment for the Wistar Institute. The two collections formed the basis of the Institute and its Museum. From this beginning came one of the primary organizations for furthering physical anthropological research. The Institute has for many years been a principal funding agency for anthropological research, coordinated many projects and enterprises, and published several journals, including the American Journal of Physical Anthropology.

The American Association for the Advancement of Science is a broadly interdisciplinary organization which, since its founding in 1848, has promoted a broad range of scientific research. Section H of the Association provides for the needs of anthropology. A variety of other organizations formed through the second half of the nineteenth and the beginning of the twentieth century have contributed to American physical anthropology. The American Anthropologist, when first published, was the organ of at least four such associations, and as such has contained considerable studies directly relating

to physical anthropology. Among the societies promoting the journal were the Anthropological Society of Washington, the American Ethnological Society of New York, the American Anthropological Association and the Philadelphia Anthropological Society. The American Anthropologist was published from January 1888 through December 1898 by the Anthropological Society of Washington, the eleven volumes constituting the first series. The second series, beginning January 1899 and continuing through the present as a major anthropological journal, has been coordinated by the American Anthropological Association. This association has also published Memoirs since 1905. A minor bulletin has also been published, first as the News bulletin, then the Bulletin, and finally, continuing to the present, the Fellow newsletter. These publications relate indirectly to physical anthropology, since they serve anthropology at large and deal more specifically with cultural anthropology.

It would be a fault of omission to exclude discussion of phrenology, which became popular in the early nineteenth century, and which certainly had a measure of influence on early anthropologists. Phrenological societies were established at Boston and Washington during the 1830's, and attracted a great many physicians and other scientists. Large collections of human skulls were assembled to satisfy the interests

of phrenologists, who had begun to investigate the human brain as the diversified seat of all bodily functions. The Boston Phrenological Society published in 1835 a catalogue of 416 specimens which reportedly were derived from the collections of Dr. Spurzheim and J. D. Holm. In due time these collections became available for more scientific endeavors.

Phrenology, although it enjoyed a day of respectability before its deneument, was not unequivocally accepted by all men of science, even those whose inclination was to accept its basic premises. Samuel G Morton, for example--the founder of American physical anthropology--in an Introductory Lecture on "The Diversities of the Human Species," explained that phrenology

> teaches us that the brain is the seat of the mind, and that it is a congeries of organs, each of which performs its own separate and peculiar function. These propositions appear to me to be physiological truths; but I allude to them on this occasion merely to put you on your guard against adopting too hastily those minute details of the localities and functions of supposed organs, which have of late found so many and such zealous advocates (Morton 1842).

Such cautious judgment on the part of the more forceful scientists of the period insured that phrenology was not uncritically accepted as scientific doctrine, until such time as the premises of the would-be science could be scientifically disproved.

John C. Warren laid much of the groundwork for the early growth of physical anthropology. The second member of the "Warren Dynasty" of medicine in Boston, he was born in 1778. In 1799, according to custom for Americans entering the medical profession, he went to Britain. There he studied with Sir Astley Cooper and gained practical experience in Guy's Hospital in London. He received the M.D. at Edinburgh, after which he went to Paris to study with Corvisart and Dupuytren. Upon returning to the United States he became Adjunct Professor of Surgery and Anatomy at Harvard. Upon his father's death in 1815, Warren assumed the full professorship (Packard 1942:100).

As an anatomist and surgeon, Warren developed interests in the skeletal remains of the American Indian. He assembled a collection of crania and mummies of various races, including Indian and Egyptian specimens. We have evidence that Warren lectured occasionally on his anatomical and anthropological studies, although it seems he had too little time to devote to this interest. Although he remained firmly dedicated to his surgery and anatomy professorship, something of a landmark in American scientific studies was accomplished in 1822, with the publication of his Account of the Crania of Some of the Aborigines of the United States, the first American publication of its nature. Several years later Warren contracted

Henry R. Schoolcraft for the purpose of collecting Indian crania. The material which Schoolcraft collected was eventually placed in the Warren Anatomical Museum of Harvard University, a museum founded by Warren.

Warren's interest in Indian crania is somewhat enigmatic today. Hrdlicka suggested that Warren's interest in the subject was "Inspired evidently by Blumenbach's works" (Hrdlicka 1919b:31). Certainly Warren did not pursue the subject to any great extent, and his Account of the Crania... was published as an appendix to his Comparative View of the Sensorial and Nervous Systems in Man and Animals. Hrdlicka pronounced the Account of the Crania... "of no permanent value scientifically" (1919b:31), despite its systematic and technical descriptions of the specimens. But Warren's contribution to the emergence of American physical anthropology must not be overlooked. His dedication to collecting, preserving and describing specimens was a firm initial step in the development of a new field. It was the enlargement upon his procedures, accompanied by refinement in techniques, which later indicated the magnitude of his contribution.

It has been suggested that the father of American physical anthropology was Dr. Samuel George Morton, and that physical anthropology in the United States begins, strictly speaking, with his work (Hrdlicka 1919b:32, 41). Morton's

personality and contributions were distinctive, yet there is some factor in his background which is at least roughly characteristic of the group of men who were the early anthropologists. His life and work, therefore, bear closer examination.

Born January 26, 1799, in Philadelphia, Morton's early training conformed with the teachings of the Society of Friends. Henry S. Patterson wrote a Memoir which was published three years after Morton's death in a memorial volume of Morton's inedited papers, called Types of Mankind: or, Ethnological Researches, based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races, and upon their Natural, Geographical, Philological, and Biblical History: illustrated by selections from the inedited papers of Samuel George Morton, M.D., by J. C. Nott and George R. Gliddon. In this work Patterson relates that Morton's most intense childhood interest was history, and that this predilection perhaps prepared the groundwork for his anthropological interests (Patterson 1854:xxii). Destined for the mercantile business, he redirected his future and undertook the study of medicine. Under the study of Dr. Joseph Parrish, a famed medical doctor, and Dr. Richard Harlan, who was also devoted to natural history, Morton received the M.D. in March, 1820, and was elected a member of the Academy of Natural Sciences of Philadelphia in April of that year.

Soon after receiving his degree, however, Morton departed for Europe, where a number of friends persuaded him to study for a European degree. Since American academic credentials were not well received in Europe at that time, Morton first had to pursue a full undergraduate course before undertaking medical study (Patterson 1854:xxiii-xxiv). Having already studied under such scholars as Wistar, Physick and James, however, he found his courses easy, and also mastered French and Italian and pursued geology under Professor Jameson. Following a year of clinical study in Paris and a summer in Italy, he returned to Edinburgh for further medical study.

Returning to Philadelphia in 1824, Morton began his career as a medical practitioner. He also began participating actively in the Academy, both in its business and social functionings and through scientific contributions. The Academy was to be a lifelong interest of Morton, as evidenced by his position as its president at his death. He began delivering papers to the Academy in 1827, first in geology, then also in paleontology. A list of his publications illustrates that Morton possessed an encompassing mind, geared both to the pursuit of isolated and specialized research topics and the more synthetic or summarizing type of study. The culmination of his early scientific interests was the Synopsis of the Organic Remains of the Cretaceous Group of the United States

(1834). Much of this work is original, reporting and describing specimens not previously recognized.

Morton maintained a constant interest in every department of the Academy, contributing regularly to the geological, mineralogical and paleontological sections. While practicing medicine in Philadelphia, he provided firsts in medical diagnosis. From 1839 to 1843 he held the Chair of Anatomy in the Medical Department of Pennsylvania College, and was a physician and teacher at the Alms-house Hospital. He was also a fellow of the College of Physicians.

This was the background, then, for Morton's entrance into matters more specifically relating to the origin and history of man. It has been given in some detail because it is broadly descriptive of the background of many of the early anthropologists. By about 1840, Morton's interests in anthropology and "Comparative Cranioscopy" were fast developing. The proposed object "being the determination of ethnic resemblances and discrepancies by a comparison of crania (a study which was then called ethnology)... the work could not be commenced until the objects for comparison were brought together" (Patterson 1854:xxviii). Morton had taught a course in anatomy in 1830, on skull differences shown in the Five Races of Men, and found that skeletal material was virtually unavailable. He determined to assemble a cabinet of crania,

therefore, which would be universal in scope (Morton 1848: 217-218). He adopted every legitimate means of acquiring skeletal material, sparing no efforts in time, labor or money. He had many collaborators, a factor which involved extensive correspondences. It is a tribute to Morton that, due to his magnetic personality and the general interest of his undertaking, people from around the globe went to great lengths, exerting great effort to obtain crania for him. The personal cost to Morton of this undertaking has been estimated at ten to 15 thousand dollars, much of which was used for expensive transportation costs (Patterson 1854:xxx).

Within a relatively short time Morton was able to assemble the largest, most complete, and certainly the most valuable collection of crania in existence. Patterson (1854:xxx), a friend and fellow scientist of Morton, reports that, at Morton's death, the collection contained 918 human crania and that 51 more, in transit at the time, arrived after his death. In addition he had accumulated 278 crania of mammals, 271 of birds, 88 of reptiles and fishes, making a total of 1656 (sic) skulls. We note, incidentally, Hrdlicka's conflicting report that 968 crania were collected, and that 67 specimens were added to the collection after Morton's death (Hrdlicka 1919b: 33). In any case, a large collection had been assembled, representative of most of the major groups of peoples of the

world. The collection was to attract considerable European interest, and would soon provide the basis for monolithic studies by Morton. In time, however--as we shall see--the collection fell into neglect and thus missed its potential exploitation.

Ethnology during the early and middle parts of the nineteenth century was conceived generally as the study of mankind, with particular interest in the separateness of races, and the demonstration of racial varieties through cranioscopy. Hence Patterson's "desire to present Morton as the Anthropologist, and as virtually the founder of that school of Ethnology, of whose views this book may be regarded as an authentic exponent" (1854:xviii). This use of the term ethnology was, in so far as can be determined, roughly accepted by most scholars of the physical anthropology of man during the mid-nineteenth century. Patterson, writing in 1854, relates that the science was a new one, of only several years' age. He reports a sudden increase in interest in "the curious diversities of form, feature, complection, &c., which characterize the different varieties of men." The groundwork for the new science (and it might be remarked that the new science of ethnology actually corresponds in great part with the more general development of American physical anthropology, and hence we are speaking of a single movement), was prepared by early

European students of cranial studies and racial differentiation, such as Jomard, Camper, Blumenbach, Lawrence, Prichard, Gall and Spurzheim. These were the men whose works directly stimulated Morton and other American investigators of the period.

Patterson suggests that "Ethnology should be eminently a science for American culture" (1854:xxxii), since three of the five races of man, as prescribed by Blumenbach, were found on the American continent. The American workshop--the melting pot--in other words, was a natural biological laboratory. Patterson is referring, incidentally, not only to the native American Indians and the large European Caucasoid population, but also the Chinese immigrants to California and the Negroids in the American South who were brought in from Africa. Morton had become a master in the scientific handling of these peoples. Integrating the studies and interests of zoology, archaeology and philology, and emerging with ethnography, his stature assumed a magnitude which led Professor Retzuis of Stockholm to write to him, on April 3, 1847: "You have done more for Ethnography than any living physiologist" (Patterson 1854:xxxiii).

While Morton's collection was becoming a reality, Morton himself was busy studying each new specimen which arrived. It was customary for Morton, when faced with a skull which he could not understand, or which differed from his expectations

or the indications of its recorded provenience, to keep the skull in his office, and to handle and study it for great periods of time, gradually forming his opinion. In this manner of careful and critical study, always calculated and considered, he built the data for his monumental Crania Americana, a magnificent volume published in 1839 with the advance support of only 15 subscribers--a remarkable feat for that day. Working under disadvantages of poorly standardized data collecting procedures, and relatively unrefined instruments, he examined and compared all of his crania from North and South America. He concentrated efforts on measuring the average cranial capacity and described his own method for accomplishing the task. In this work he introduced his ethnological views, through an interpretational comparison of the crania and determination of the original racial character of American populations. He formulated two basic ethnological decisions, both of which are significant in anthropological interpretation and the history of anthropological thought. First, the American nations, with the exception of the Eskimo, "were of one Race and one Species, but of two great Families (Toltecan and Barbarous)." These groups resembled one another physically, but differed intellectually, according to Morton. Second, the remains of the Mounds, from Peru to Wisconsin, were members of the same Indian people, probably the Toltecan

family. Morton's interpretation of the homogeneity of American peoples was striking in its temporal-historical context. Popular thought ascribed a great deal of diversification to the native peoples of the Americas, and considerations of their origins were equally diversified. And we cannot help noticing Morton's remarkable astuteness in ascertaining--despite the tremendous gaps in knowledge and information--that all the American peoples might be related, with the exception of the Eskimo. For evidence today indicates that the Americas may indeed have been peopled from a single basic genetic stock through, perhaps, a series of waves of immigration or a continued influx of people. Stewart (1960) has presented an excellent review of the question, and suggests a unified origin for the American Indians. Newman (1951, 1953) had previously discoursed on the subject and reached similar conclusions, as had Hrdlicka before. (Note, however, that Comas, 1960, does not accept this postulate and has advanced a number of arguments against its acceptance.)

While his Crania Americana was materializing, however, Morton was working on another similar project. It is not specifically American in scope, but serves to illustrate the diversity of his interests and abilities. Having contacted George R. Gliddon, a U S. consul in Cairo, Morton began to receive, through Gliddon's enterprises, crania and mummies

from Egypt. The eventual result was Crania Aegyptiaca, published in 1844. In this carefully considered work, he provided interpretations on the races of Egypt and surrounding areas.

At a time when it was scarcely fashionable, much less advisable to question Biblical chronology, Morton approached the question. It was becoming increasingly apparent, both in Europe and in America, that those who would understand the nature of man and his past must go beyond the dictates of the Church and the Ussher chronology. Being a Quaker by birth, and a "true disciple of the inductive philosophy" (Patterson 1854:xlvii), Morton moved slowly and deliberately. He waited patiently for more evidence, and in due time published his beliefs. In Crania Americana and elsewhere he put forth his views on the permanency of race, uninfluenced by any significant extent by physical influences, and greater in scope and diversification than could be subsumed in the time allotted by the Bible. He subscribed to the idea of original human diversities, basing his beliefs mainly on morphological observation.

Morton's life and career--quietly dynamic, in a sense--were cut short by his death, at the age of 52, on May 15, 1851. His legacy was immediately apparent, however, in his cabinet of crania, his monumental monographs, and, finally, in his influence upon a new discipline.

Four individuals were directly responsible for carrying on Morton's work, each in a different capacity. Upon Morton's death, his collections and cranial investigations were offered as a continuing project to Joseph Leidy, M.D., LL.D., a vertebrate paleontologist who was then Curator of the Academy of Natural Sciences of Philadelphia. Leidy, due to his own researches at the time, did not accept the opportunity. Instead, when he, as Curator, assumed charge of Morton's collection, which had been purchased and donated to the Academy, he offered the undertaking to J. Aitken Meigs. In the years which followed, however, Leidy produced at least seven studies on human anatomy and osteology. These relate to cranial bones and to fossilized skeletal material.

J. Aitken Meigs, a professor of climatology and physiology, accepted Leidy's offer regarding Morton's researches. According to Hrdlicka (1919b:41-42), Meigs attempted to continue where Morton had left off, and during the ensuing 16 years contributed a number of good papers. He continued publishing Morton's Catalogue of skulls, and provided studies on cranial mensuration. His Mensuration of the Human Skull (1861) gives directions for 48 cranial measurements and determinations. Through a chance course of events, therefore, Meigs became a physical anthropologist and began contributing to the growth of the new science. After Meigs' studies,

Morton's collection of crania remained in the Academy and gradually fell into disuse.

J. C. Nott and George R. Gliddon, who were close associates of Morton, and who have been mentioned earlier in this chapter, furthered the work of Morton. After Morton's death, they acquired his personal and scientific papers and edited them into a large volume on the Types of Mankind (1854). This volume incorporated not only some of Morton's unpublished researches, and Nott's and Gliddon's studies, but also contributions by Louis Agassiz, W. Usher and H. S. Patterson. Unfortunately, many of the more than 700 pages were spent on reconciling scientific findings with biblical chronology and tradition. Nevertheless the work contains much valuable information, especially of an historical nature.

Jeffries Wyman was another key figure in the formulation of American anthropology and archaeology. Wyman was born in 1814 and, studying at Harvard, received a medical degree. He was for a time Demonstrator of Anatomy at Harvard College. In 1840 he was appointed Curator of the Lowell Institute, and in 1840-41 delivered a series of 12 lectures on comparative anatomy and physiology. The money gained enabled him to go to Europe for further study. In Paris he studied comparative anatomy and physiology and undoubtedly had some contact with the beginnings of physical anthropology (Hrdlicka 1919b:46-47).

In 1843 he began teaching at Hampden-Sidney College, Virginia. In 1847 he succeeded Dr. Warren as Hersey Professor of Anatomy at Harvard College.

In 1852 Wyman began a trip South, exploring shell-mounds in Florida. In 1856 he traveled to Surinam and explored there. In 1858 he traveled extensively through Argentina, crossed the Andes into Chile, and returned via Peru and Panama. When in 1866 he found himself unable to continue teaching, George Peabody named him a trustee of the newly-founded Peabody Museum of American Archeology and Ethnology at Harvard University. During his tenure as Curator, cranial collections were assembled, including some Peruvian specimens, forming the basis of the extensive collections which the institution now holds.

While functioning as Curator, Wyman continued to pursue his own research interests. His anthropological bibliography reveals that he conducted several studies on the gorilla and chimpanzee. According to Hrdlicka (1919b:47) he gave us our first precise osteological knowledge of the gorilla. He published "Observations on the external characters, habits, and organization of the Troglodytes niger" (1843-44), a "Notice of the external characters, habits and osteology of Troglodytes gorilla, a new species of ourang from the Gaboon river" (1845-47), "A new species of Troglodytes" (1848), "A

description of two additional crania of the enge-ena (Troglodytes gorilla, Savage and Wyman) from Gaboon, Africa" (1849), "Dissection of a black Chimpanzee (Troglodytes niger)" (1854-56), "Account of the collection of gorillas made by Mr. Du Chaillu" (1860), "On bones of a gorilla recently obtained in western equatorial Africa" (1861) and "Observations on the cranium of a young gorilla" (1863). These primate studies are among the first such investigations, of high calibre scientifically, to appear in the New World. They certainly represent an awakening to new research potential, as well as the beginning of a new research trend. Other anthropological investigations which Wyman accomplished are varied in nature, and relate chiefly to osteological questions. He made several investigations into symmetry and malformations of the human body (Wyman 1864, 1866, 1868).

Wyman's successor as Curator of the Peabody Museum was Frederick Ward Putnam, who has been characterized as "one of the best friends and promoters physical anthropology has had in this country" (Hrdlicka 1919b:49). Putnam, born in 1839, had a most inadequate education, but was so inclined to natural history that by the time he reached 17 he became Curator of Ornithology at the Essex Institute of Salem. Between 1857 and 1864 he was under the direct influence of Agassiz and continued his education at Harvard. In 1864 he became Curator

of Vertebrates at the Essex Institute. He was a founder of the American Naturalist, a journal which has rendered invaluable services to anthropology. In 1873 he was elected Permanent Secretary of the American Association for the Advancement of Science. He held the position for 25 years, when he became President of the Association. He also became a member of the National Academy in 1885.

Putnam demonstrated anthropological interests at a rather early date in his career. His first paper, on Indian graves, dates back to 1865. He became Curator of the Peabody Museum in 1875. In 1886 he was made Peabody Professor of Archeology at Harvard and held the position until 1909, when he retired as Professor Emeritus. During the World's Columbian Exposition at Chicago, he served as Chief of the Department of Ethnology. Here he worked with Boas, and was instrumental in assembling the collections which were to become an integral part of the Field Museum of Natural History.

Although Putnam was much more an archaeologist than a physical anthropologist, he did much to aid the growth and development of the latter. He studied American Indian osteological remains, and also became interested in the antiquity of man in America. He was inclined to accept an early date for man's presence in America, but seems to have offered no serious obstacle to Hrdlicka's dominant position on the subject (see Hrdlicka 1919b:50).

Henry P. Bowditch (1840-1911), as Professor of Physiology at Harvard Medical School, produced a number of physiological studies which were of importance to early anthropologists. In addition he conducted researches which were even more directly physical anthropological in nature. He published in 1877 a key study on the "Growth of children." Attempting "to determine the rate of growth of the human race under the conditions which Boston presented," he studied 24,000 Boston school children, from five to 19 years of age. He found that children in private schools, representing well-to-do families, had taller and heavier body builds than did those children in corresponding grades of public school. Hrdlicka reveals (1919b:52) that Bowditch's growth studies were stimulated by researches on Belgian children which were published in Quetelet's Anthropometrie (Brussels, 1870). The literature indicates that Bowditch's study on Boston children was among the first growth and constitution studies to be conducted in the United States. His physiological interests led him further to investigate the "Relation between growth and disease" (1881), "The physique of women in Massachusetts" (1889-90), and other constitutional questions. His studies, innovative though they were, provided a great deal of knowledge about growth rates of children and about the physiology of the population in general. They were the stimulus for all later growth and constitution studies in this country (Hrdlicka 1919b:52).

A man instrumental in the pre-1900 development of American physical anthropology, but whose name is seldom heard today, was Harrison Allen. Allen was born in 1841 and received only an inadequate education. Teaching himself, however, he was able to enter medical study at the University of Pennsylvania and received a degree in 1861. In the immediately following years he practiced medicine and surgery. Beginning in 1865 he conducted research in anatomy, anthropology and biology, in addition to his medical practice. At the University of Pennsylvania he became Professor of Zoology and Comparative Anatomy in the auxiliary Faculty of Medicine. He became President of the Association of American Anatomists in 1892, and shortly thereafter was appointed Director of the Wistar Institute.

Allen produced a number of studies which are strictly anthropological in nature although, as Hrdlicka has suggested (1919b:59-61), Allen's studies were not always reported with depth and thoroughness. Although this shortcoming is to be regretted, it is noticeable that Allen conducted a wide variety of studies and illuminated a number of subjects, particularly cranial, which were previously unknown. His first major publication was The Clinical Study of the Skull (1890), a lecture first delivered in 1889. It deals with morphology and anomalies of the skull, many of which had not previously

been discussed. His memoir on Crania from the Mounds of the St. John's River (1896) is likewise a carefully considered study of notable scientific merit. Here Allen demonstrated several new cranial measurements, discussed in particular the malar bone, and discussed dentition and the lower jaw. The Study of Hawaiian Skulls (1898) was completed just before his death and occupies something of a transitional position in the literature. In scope and quality it is a modern study, comparable also with European studies of the same period (Hrdlicka 1919b:59-60). Besides many cranial observations, Allen gave attention to graphic representation of the data, with a highly creditably study as the result.

Allen's stimulus to pursue anthropological questions came from Morton and from Meigs, who was a personal friend. He also followed similar studies which were being conducted in England. It is surprising, in light of his associations, his academic position, and his scientific studies, that his name has not been more enduring to the present day. It is strange that, despite his stature, he has had only moderate influence on the discipline. The explanation has been attributed to the fact that rather than giving himself wholly to physical anthropology, Allen was also devoted to biology and anatomy (Hrdlicka 1919B61). In addition, Allen never taught; hence he had no students to carry forth his ideas.

This survey of the early development of American physical anthropology indicates that although there were some teachers among the early scientists, the essential orientation was toward research. This factor is partly due to the backgrounds and interests of these men. That is, they were researchers from the beginning, and merely began extending their investigations in the direction of a burgeoning new science.

One of the most important general anthropologists in the history of the discipline made numerous specific contributions to physical anthropology. Franz Boas was born in Minden, Westphalia, in 1858. He was a student at the Universities of Heidelberg, Bonn and Kiel, from 1877 to 1881. He received his Ph.D. from Kiel in 1881, and M.D., honoris causa. He received LL.D. and Sc.D. degrees from Oxford, Clark, Howard and Columbia Universities.

Trained in physics, geography and geophysics, Boas joined an expedition to Baffinland in the early 1880's. His attention gradually turned from the search for environmental effects on Eskimos to the importance of social tradition in determining cultural and individual behavior. Hence he became active in recording the ethnology and anthropology of Eskimo and Canadian Indian tribes. In 1888 he was appointed Docent of the sub-department of Anthropology of Clark University. Hrdlicka reports (1919b:86) that Clark University was one of

the earliest foci of American physical anthropology and the first to confer the Ph.D. degree. Early in his career, therefore, Boas found himself in a strong anthropological atmosphere--an excellent forum for expounding on his anthropological knowledge and ideas. From 1891 to 1894 Boas served as Chief Assistant of the Department of Anthropology of the Chicago Exposition. During this time he conducted somatological and growth studies. In 1894 he joined the American Museum of Natural History as Assistant Curator in Ethnology. In 1903 he was to assume F. W. Putnam's position as Curator of Anthropology. At this point, however, Boas' interests centered on ethnological (in the modern sense) collections, and physical anthropology at the Museum fell into neglect (Hrdlicka 1919b:98).

In the meantime Boas had begun teaching at Columbia University. He became Lecturer on Physical Anthropology in 1896, and Professor of Anthropology in 1899. He taught an introductory course, with lectures, essays and discussions, as well as a more advanced course. The latter included statistical and biological problems, with lectures, reports and laboratory work. In 1905 Boas left theAmerican Museum and devoted his activities more fully to the Department at Columbia. His researches and published contributions are numerous and vary widely in subject matter. His physical anthropological studies were, in general, oriented more mathematically

than somatologically. This was perhaps a result of his background in the physical sciences.

Perhaps Boas' most significant contribution to anthropology lay in his foresight. Envisioning a rapid disappearance of the American Indian, due to the explosive encroachment of European groups on this continent, he set about to recover as much data and information as possible. In many respects, Boas was a salvage anthropologist. The critical nature of this orientation, however, did not detract from the scientific and professional merit of his studies. They are of highest calibre.

We notice, then, that Boas was both a teacher and a researcher. With regard to this duality, Lowie, in The History of Ethnological Theory (1937), suggested that Boas' historical position was unique. He was the first anthropologist to successfully combine extensive field investigation "with an unrivaled opportunity to train investigators" (1937:129). While the magnitude of Boas' contribution to American anthropology is seen in his researching capacities, therefore, it is also reflected in his legacy--his students. As a teacher he was unwilling to involve himself with other people. He established relationships with his students on the basis of the merit of his work, but never as a result of personal magnetism or academic showmanship. And rather than attracting a

large crowd of uncritical, enthusiastic followers, Boas worked with a small number of devoted students (Kardiner and Preble 1961:124). It is one of many paradoxes of Boas' life that he avoided any artifice or situation which would attract students, and simultaneously produced a great number of anthropology students (Lowie 1937:151), among them R. H. Lowie and Ruth Benedict. The number was small, by modern standards, but large with regard to previous anthropology training programs.

Despite the core of students who carried forth Boas' work and his anthropological method, destined to enshrine him as possibly the greatest anthropological teacher, there is no Boas "school" of anthropology (Kardiner and Preble 1961:121). This paradox is a result of his refusal to see his ideas systematized or codified. Lowie suggests (1937:152) that the systems-oriented person cannot understand a scientist's ability to investigate problems without producing generalizations from his data; he overlooks the scientist's conviction that systematization may be premature, and best left to the philosopher. A consequence of this compulsion to avoid systematization was inconsistency in much of what Boas professed through the years. His open-mindedness, receptivity to new ideas and possibilities, and his willingness to change, resulted in continually emerging interpretations and opinions. What some have called inconsistency, therefore, is actually only the by-product of a progressive mind.

Boas was an independent thinker, a lifelong opponent of authority and systematization, and a factually-oriented scholar. Kardiner and Preble note (1961:121) that he fought all authority, especially when personified by a university administrator. While Curator of Anthropology at the Field Museum, for example, personal conflicts developed and Boas was subsequently removed from the position. During World War I he was a pacifist, a position which placed him at the center of controversy. Since loyalty to country was a major concern of the American public and the political heirarchy, Columbia University--where Boas had been teaching for some time--encouraged reports from students on any suspicious statements and activities made by the faculty. Boas, rather than circumventing the situation, which he surely considered oppressive, prepared a statement on his views and offered copies to students who were interested in reporting his sentiments (Kardiner and Preble 1961:122). He was never really charged with disloyalty to the United States, but rather was castigated for his attachment to his native land, Germany. Before the outbreak of World War II, however, he was among the first to take a stand against the racist views of Hitler and his regime. This led to the burning of his books in 1933 at Kiel.

Boas demanded freedom from authority not only for himself, but also for his students. He encouraged them to think

independently and to fight authority, even his own. He carried this belief to the extent that "when any of his ideas were threatened with systematization he went off on another tack, leaving his followers without a flag" (Kardiner and Preble 1961:121). Working inductively, in the field and in the laboratory, Boas faithfully avoided making assumptions and generalizations. His monographs are written plainly and cryptically, devoid of feeling and creative literary polish. He focused on description, believing that emotional involvement with anthropological subject matter detracted from the scientific merit of the product. His orientation toward his research was skeptical and analytic, but always reserved. It is significant that the only motto to which he is known to have subscribed is "Icy enthusiasm," a phrase he supposedly adopted from the great pathologist Rudolf Virchow.

Several paradoxes characterize Boas' life and work. Lowie (1937:130-131), however, has introduced a more encompassing appraisal relating to Boas' career in general, accounting for the would-be ambiguities. Boas was a renowned scholar who enjoyed achievements of the highest quality. He was one of the greatest figures in international science. His influence on the world, however, did not correspond to his intellectual stature. Lowie suggested several factors which may explain this paradox. First, Boas was a researcher

but not a publicizer of his work. Second, he did not direct his attentions toward educating the masses, nor did he appeal to cultivated and aesthetic interests. Third, he wrote only monographs, but not books. Finally, Boas produced neither a condensation of his work, nor a "soul-stirring scientific message." Lowie suggests, in conclusion, that

> Those ethnologists who crave generalizations are certainly doomed to disappointment. Boas' greatness lies not in the systematic elaboration of facts, but in his independent approach to that material, his novel classification of it, his capacity for defining problems hitherto undreamt of, his insistence on a methodologically rigorous solution (1937:155).

Only one qualification of Lowie's assessment might be ventured. That is, not only ethnologists but all anthropologists might be addressed by this statement, for it sheds light on all of Boas' research.

Boas' career was long and full, including many researches, explorations and academic positions which are not recounted here. He worked in Mexico from 1910 to 1912, and was named an honorary professor at the National Museum of Archaeology in Mexico. He was Corresponding Secretary of the Germanistic Society of America, and a member of the National Academy of Sciences, American Philosophical Society, American Antiquarian Society, and American Folklore Society (Editor 1908-1925, President 1931). He was a Fellow of the

American Association for the Advancement of Science (Vice President 1905, 1907, President 1931), New York Academy of Sciences (President 1910), American Anthropological Society (President 1907, 1908), and American Academy of Arts and Sciences. He died December 21, 1942.

Among the first of the great teachers in the discipline was Earnest A. Hooton. Hooton was born in Clemansville, Wisconsin in 1887. He received theB.A. degree from Lawrence College, Appleton, Wisconsin, in 1907, an institution which also awarded him the Sc.D. in 1933. He received his M.A. from the University of Wisconsin in 1908, and his Ph.D. in 1911; he was awarded an honorary LL.D. in 1954. From 1910 to 1913 he was a Rhodes Scholar at Oxford University and received his diploma in anthropology in 1912 and the B.Litt. in 1913.

Joining the Peabody Museum at Harvard University as Assistant Curator of Somatology in 1913, he was appointed Curator in 1914. Much of Hooton's career was devoted to the teaching and researching of somatology, although his interests carried him afield at times from this specialization. At Harvard Hooton taught general and advanced physical anthropology, criminal anthropology, and race mixture. He utilized teaching methods whereby students conducted original research. Although his seminar classes were small, some of

his larger lecture classes included up to 100 students. It is apparent, therefore, that he reached a rather large number of students, in addition to his tutorial students--considerably more, at least, than had been reached by previous anthropologists. In 1917 he was named Fellow in Anatomy by the Harvard Medical School, and thereafter taught anthropology to medical students also. Among his students were some who are well-known in anthropology today: J. B. Birdsell, C. S. Coon, S. M. Garn, W. W. Howells, F. S. Hulse, E. E. Hunt, G. W. Lasker, M. T. Newman, H. L. Shapiro, C. E. Snow, S. L. Washburn.

Hooton's light-hearted, tongue-in-cheek attitude toward anthropology, teaching, and students shines through in Apes, Men and Morons, first published in 1937. Able to transcend the stuffy academic professionalism of so many early scientists, Hooton approached his subject as the very human study which it is. Communicating in the classroom or on the printed page, he was able to translate evolutionary theory into readily understandable concepts and phraseology, sprinkling his work along the way with a genuinely unique humor. He was particularly adept at limericks, which very graphically and effectively demonstrated scientific points.

Apes, Men and Morons is basically an introductory volume to the subject matter of physical anthropology. It is analytic and interpretative, however, rather than descriptive.

Addressing himself to the public, he approached questions which society asks and anthropology can answer. Part III of the volume was devoted to the need for evolutionary guidance. Hooton believed in the possibilities of eugenics, realizing full well that his views were not generally popular with other physical anthropologists. In typical Hooton fashion, however, he put caution aside and followed the dictates of his conscience. Up From the Ape, published in 1932, also reflects his ability to relate his subject to the public. He says in the Preface, "This book... is intended primarily for the more or less educated layman and not either secondarily or more remotely for the overeducated professional. Indeed I rather hope that not many of the latter class will read it..." (1932:viii). The work is essentially an introductory text for physical anthropology and was widely used as such by many physical anthropologists for over 20 years.

Hooton's most important and best-known scientific monograph was The Indians of Pecos Pueblo, published in 1930. The work deals with the large collection of human skeletal material recovered at Pecos, being a comprehensive osteological investigation. The thoroughness of the monograph is such that it has become a standard reference work not only with regard to the southwestern Indians which it treats, but also as a model for the effective reporting of a large series

of skeletons. As a descriptive, analytic and interpretational work, The Indians of Pecos Pueblo was a valuable addition to the literature of physical anthropology.

Hooton published many other articles relating to somatology, and had parallel interests in racial characteristics and classification. His stature as a somatologist is indicated by his position on the American Association of Physical Anthropologists Advisory Committee on Anthropometric Interests in 1935. The purpose of this committee was to revise anthropometric techniques, and generally to seek a basis for international agreement and standardization. Anthropometry, as a technique established through over half a century of use, was poorly organized and had no systematic structure. Standardization of measurements--which, obviously, depends upon voluntary and common acceptance--was in many respects a goal but not a reality. Anthropometry requires precision in a number of respects. Comparative data cannot be obtained or used if individual investigators do not subscribe to the same techniques. Thus, this Committee and others sought to reach agreements on procedure.

The work of the Italian criminologist Cesare Lombroso (1835-1909) on crime and man intrigued Hooton and stimulated him to investigate further the possible correlations between body build and predisposition to criminal activities.

Lombroso had studied both the anthropological and psychological nature of the criminal (1887, 1891, 1892, 1895; Lombroso and Ferrero 1895), as well as the causes and remedies of crime (1911). He postulated a hereditary basis for antisocial conduct, specifically that criminals show many physical anomalies which may be either atavistic or degenerative. The atavistic theory implies that the criminal, in some traits, reverts to a primitive type of man, while the degenerative theory suggests that the criminal may be the result of diseased ancestral stocks which have survived despite their non-progressive evolutionary tendencies. In summary then, Lombroso and his followers held that crime was a sort of abnormal biological phenomenon.

Hooton reviewed these studies and others in the presentation of his extensive research reports--The American Criminal, An Anthropological Study (1939b), his scientific monograph on the subject, and Crime and the Man (1939a), a book written for the public. Disagreeing with much of Lombroso's logic and theory, particularly with respect to the dichotomous atavism-degeneration diagnosis, Hooton sought to shed more light on the subject, for while the thesis that the criminal deviates psychologically and anatomically from the normal person was not conclusively demonstrated by Lombroso, neither had it been refuted (Hooton 1939b:17).

Hooton's sample, taken from jails, prisons, reformatories and asylums, numbered approximately 17,000. His research and search for criminal records led him to many localities across the nation, including Texas, North Carolina, the Southwest, Tennessee, Kentucky, Wisconsin and Massachusetts. He and his staff took some 20 measurements--with pre-standardized techniques and equipment--and calculated 13 indices. These were statistically analyzed and cross-compared in a variety of ways--by geographical locality, by indice, etc. Other sociological factors were noted and recorded, and subsequently integrated into the study. The study was, obviously, innovative in many respects. The scope and depth of the project certainly exceeded any previous attempts at criminological investigation. The statistical analysis is thorough, and impressive when the general state of statistics at that time is compared with the knowledge of today. The work was not, however, accepted with equanimity by most anthropologists. Merton and Montagu (1940) pointed out a number of objections to Hooton's method, procedure and analysis.

Hooton was a Fellow of the American Association for the Advancement of Science (Vice President of Section H, 1923-24), the Royal Anthropological Institute, and the American Academy of Arts and Sciences. He was also a member of the American Anthropological Association, American Genetic Association,

American Philosophical Society, American Society of Naturalists, National Academy of Sciences, Phi Beta Kappa, and an honorary Fellow of the American Academy of Dental Scientists. He died May 3, 1954.

A tribute paid to Hooton by Comas is worthy of mention here. Its relevance to history and apology of the discipline is self-explanatory. Comas reported that, "Until 1953, and we don't believe there have been any changes since then, only one course in the 'applications of physical anthropology' was being given at a major American university" (1960:660). Hooton, until his death, taught applied physical anthropology at Harvard. He was the first to explore, with tangible results, the relationship between anthropologists and anthropological data, on the one hand, and industrial, military and general human needs on the other. This was the inauguration of yet another area of physical anthropological investigation--one which made practical application of anthropological data, to the extent that clothiers, furniture makers, aircraft manufacturers and other industrial designers could more perfectly accommodate the human body. A concommitant development permits the public to buy by size, with some assurance that they will be properly accommodated.

No study of American physical anthropology would be complete without a consideration of the life and work of Ales

Hrdlicka. Indeed, as the present manuscript has witnessed, it is virtually impossible to discuss developments in the field without encountering Hrdlicka. He was a self-made man in the finest American tradition. Born in the city of Humpolec, Bohemia, on March 29, 1869, he attended elementary schools there until the age of 13, when his parents immigrated to New York. There he worked in a tobacco factory for six years until 1888, when he fell ill with typhoid fever. His attending physician at the time, Dr. M. Rosenblueth, a former Jewish Rabbi, suggested that he undertake the study of medicine at Eclectic Medical College of the City of New York. In 1892 he graduated first in his class. Two years later, again the top student, he graduated from New York Homeopathic Medical College (Montagu 1944:113).

Hrdlicka began his long career of anthropometry when, in 1894, he accepted a research position at the State Homeopathic Hospital for the Insane in Middletown, New York. A year later he went to Europe and studied anthropometry under Manouvrier in Paris. Returning to New York in 1896, he joined the Pathological Institute of the New York State Hospitals. Here he began applying anthropometry systematically, accumulating data and publishing. He resigned from the Institute in 1899 and accepted a research position as physical and medical anthropologist on one of the Hyde Expeditions for the

American Museum of Natural History, a position he held for three years.

His life assumed a new direction in 1903 when he was appointed Assistant Curator in charge of the new Division of Physical Anthropology at the National Museum. He became full Curator in 1910, a position which he held until resignation in 1941. From the day of his installation at the National Museum, Hrdlicka began amassing collections and studying them with an uncanny drive. The skeletal collections of the Division are among the most extensive and most valuable anywhere, and are largely due to the singlehanded efforts of Hrdlicka (Montagu 1944:114). Always basing his studies on the anthropometric technique, Hrdlicka began to go beyond description and gross morphological studies. He began integrating his work through probing questions of the origin and evolution of man. His particular interest was the antiquity of man in America. He was unequivocally convinced that man was a newcomer to the New World, that certainly no more than 10,000 years could be allowed for man's dispersal over the two continents. Two of his major works, Early Man in South America (1912) and Discoveries Attributed to Early Man (1918), were directed toward the illustration of his stand. Both volumes are thorough and critical reviews of the archaeology and physical anthropology of sites which are suggestive of

early occupation of this continent. He presented convincing arguments against the acceptance of these sites, however, in a typical manner which earned for him the caricature of a giant Hrdlicka standing over the Bering Strait holding back impatient hordes of people until an appropriated date.

Humor notwithstanding, Hrdlicka was firmly committed to his belief, with such force that the question of early occupation of the New World was suppressed for many years. His stature and influence were such that it was frequently inadvisable to question him. He delivered judgment ex cathedra, in a pontifical manner, a bearing which angered some contemporaries and amused others (Montagu 1944:116). His role is understandable in a very human sense, however. He was the guardian angel of a discipline which he was largely responsible for creating. His administrative and coordinating efforts brought students of American physical anthropology together and gave them direction. In addition he was possibly the most prolific writer the discipline has seen. From 1894 until his death he published an average of eight papers a year. Many of these were lengthy and several were book-length. The result is a bibliography of over 350 items.

Hrdlicka's researches were thorough and extensive, and always systematic. They are also, however, well categorized, a factor owing to his particular delineated interests. He

was not, for example, interested in math or statistics and long fought to keep such erosive and ruinous elements out of the science. Through editing the American Journal of Physical Anthropology, which he founded, he was able to discourage any significant use of statistics. His knowledge of genetics was also severely limited. Montagu says that Hrdlicka's heroes were Broca, Virchow and Manouvrier, all great European scientists with the general orientation of physical anthropology. It is observable that to a great extent he patterned his life and researches after these three scholars. He was an observer and a describer, and the experimentally-minded studies of men like Boas held little interest for him (Montagu 1944:115).

His stature among his fellow scientists, nevertheless, was unquestionable. He served as Secretary General of the XIX International Congress of Americanists (1915), Secretary of the anthropological section of the Second Pan-American Scientific Congress (1915-16), and Secretary of the Committee on Anthropology of the National Research Council (1917-18). He was a life member of the American Academy of Arts and Sciences, and was also a member of the Association of American Anatomists, American Anthropological Association (President 1925-26), National Academy of Sciences, American Philosophical Society, Washington Academy of Sciences (President 1928-29),

Archaeological Institute of America, and American Association of Physical Anthropologists (Founder, Life Member, President 1928-32). Delivering a lecture in London in 1939, before an audience including Sir Arthur Keith, J. B. S. Haldane, Geoffrey Morant, and many other notable scientists, he was introduced as "America's most distinguished physical anthropologist" (Montagu 1944:117). Thirty years later the statement remains a fitting description of the man.

In order to provide a publication outlet for the new science, and in order to contribute forcefully to the successful growth of that science, Hrdlicka launched the American Journal of Physical Anthropology in 1918--but not without due thought. The world was engaged in war. Times were hard. Education and scholarship always suffer from the effects of war. Hrdlicka recognized these facts and, in his Preface in the first issue, raised three points for evaluation: the need for an additional publication, the possible effects of a new journal upon existing periodicals, and the special services which it could actually serve. The Preface is both historical and apologetic in nature. The lack of scientific communication in the United States, for example, is noted. On the other hand, Hrdlicka envisioned the Journal contributing to the solution of massive problems inaugurated by the war. He looked toward investigation of the United States

census and an anthropological survey of the population, regulation of immigration, and eugenic progress, among others. Thus was the new journal founded. The first issue included the first section of Hrdlicka's history, "Physical anthropology, its scope and aims," "The Piltdown jaw" by G. S. Miller, "On certain Eskimoid characters in Icelandic skulls" by E. A. Hooton, and a great number of notes, communications and reports.

From the time of its inception until 1927, Hrdlicka carried full responsibility for all aspects of the Journal. In 1927 the Wistar Institute assumed all duties except editorial supervision and reviewing. Thus freed of a great burden, Hrdlicka turned to the organization of physical anthropologists into an academic and professional association. Founded for the benefit of individual scientists and in support of the Journal, the American Association of Physical Anthropologists held its first annual meeting in 1930. The meeting was, by course of events, organizational and formulatory in nature. Subsequent meetings have stressed the presentation of research reports, in addition to business and policy matters.

The Journal was reorganized in 1943. Hrdlicka, who had held the editorship to this date, turned over the position to his assistant editor and only student, T. D. Stewart.

Stewart reorganized the Journal and said editorially, in the first issue of the New Series, "If, as in the first instance, war can be said to encourage anthropological progress, then it should be an auspicious time to make this change." Stewart acknowledged the force of Hrdlicka's guidance to physical anthropology during its formative years as an organized science. According to Stewart, however, the discipline had changed through the years. Different schools of thought had arisen, and the Journal had not always incorporated new research. The primary orientation had been toward craniometry. Under the new arrangement, individual physical anthropologists would have more control over editorial policies.

As stated at the beginning, the present history is not complete. Yet it has been reconstructed in order to demonstrate a specific point: A history of American physical anthropology has been established--a history uniquely characteristic of the discipline. The transpiration of events has been largely due to the efforts of a number of people. Most notable are Hooton, Boas and Hrdlicka, for due to their efforts there was a future. The future was in the hands of their students. Their stimulating and guiding permeation of the discipline and of the education system made American physical anthropology what it is today.

CHAPTER IV

THEORY OF THE DISCIPLINE

The nature of American physical anthropology, as described in the preceeding chapters, is highly specialized, while embracing considerable internal diversity. The fact that the first anthropologists were anatomists and physicians and only in this century have Ph.D.'s assumed leadership in the field, the highly specialized and imperfectly formulated scientific pursuits of the discipline, the broad spectrum of topics appertaining to the evolution and present state of human biology, all have inhibited the development of an all-encompassing body of theory. For judging by all outward indications, American physical anthropology is peculiarly lacking with regard to a formal body of theory. There is, for example, no synthesis which may be taken to represent a coherent and logical theory of the discipline, a literary void which reflects the non-integrated state of whatever theory does exist. There are a number of summaries and minor syntheses in the literature, and these do indeed serve a most valuable purpose. But by and large they are not acceptable as an integrated philosophical statement on American physical anthropology, for they approach specific and isolated subject areas.

This is not to imply that there is no theory in the discipline, but rather that the theory is obscured. It is ingrained, interwoven with the fabric of pure research, but it is not united. Consisting of statements by anthropologists as individual research personnel it concerns their views on what anthropology is and does, and how it may or should be approached. For example, each project carried out under the auspices of the discipline is usually explained and justified, and any relevant theory is called to bear for the project at hand. Further, a number of studies have been directed toward the probing of philosophic implications and involvement of research on the biology of man. And such studies certainly have emerged as something of a unified whole, if the conceptualization may be allowed; and that whole is the theoretical framework of the discipline, if only because it is all that we have to work with. Hence, there actually is a great deal of theory in physical anthropology.

The problem, as stated before, lies in that there is no central core of theory. The theory which is present is fractionated, and in order to understand it as a workable entity or to utilize it as a scheme of ideas, one must seek it concertedly and directedly. There is a need, then, for an integration of physical anthropological theory. The discipline is growing rapidly. Specialization has tended toward

polarization in recent years, until at present the scope of physical anthropology is almost too broad to be mastered by the individual scientist. Hence summarization and standardization are needed, as keys to the orientation and direction of the future of the discipline. While this goal is hardly fulfilled in the present work--which is essentially a history rather than a text on theory--the goal of this chapter is to summarize the major theoretical trends in the American science. And the need for an encompassing treatise which integrates and unites the theory of the discipline will remain.

Chapter VI, in which the various areas of research in American physical anthropology are investigated, provides the underlying data and theory pertinent to these diverse topics. Much of the minutiae of theory has already been presented. We have examined the theoretical orientations, the goals and methods, of such early anthropological researchers as Morton, Leidy, Meigs, Wyman, Putnam, Bowditch, Allen and others. Their studies formulated the discipline and gave it direction. Their orientations provided the core--the evolutionary trunk, so to speak--for most of the subsequent history and theory of the discipline. In addition we have examined the subsequent frenzy in recording information, the desire to maximize perishable data. The late nineteenth and early twentieth centuries were something of an era of salvage anthropology.

Inherently present in this period was the applied/pure research dichotomy. There was little time or room for arguing this point at the turn of the century, although the character of research certainly was affected by divergent orientations. Whether data were recovered because they were perishable (and, in fact, perishing) or because they could serve a practical purpose was seldom debated. But the question was present and survived for subsequent appraisal.

Finally, we have seen that the technique of anthropometry, an inheritance from European scholars, occupied a central position of importance in American physical anthropology. It has been something of an orientational base. It is precisely this facet of anthropology which was so frequently to occupy the time and interest of physical anthropologists during the twentieth century. Interestingly, it seems only fitting that anthropometry should have come to be the pivoting point of American physical anthropology. For the discipline was founded on the ramifications of that technique which was developed and largely perfected in the Old World, introduced and expanded in the New World and, in the middle and late nineteenth century, offered new horizons for research. Such was anthropometry. Most of the remainder of this chapter shall be devoted to various aspects of the anthropometry question, for in all its implications and extensions

it focuses on the evolution of the discipline. It is the most central topic of American physical anthropology, around which the other sub-areas function and flourish.

The first major work in American physical anthropology of a theoretical, historical or summarizing nature was produced by Hrdlicka in the early 1900's. Physical Anthropology. Its scope and aims; its history and present status in the United States, was published in separate parts of the American Journal of Physical Anthropology, then as a monograph by the Wistar Institute in 1919. Hrdlicka's early realization of the need for integration and directed progress is notable. In his Preface to Physical Anthropology he says:

> Organized progress in any branch of science is possible only when the field of that branch becomes well defined. But a definition to be of value must in a large measure be based on experience, and that not on individual but on the collective experience of the workers in that line. The history of a given branch of science thus becomes one of the essentials to the proper comprehension of the scope, objects and demarcations of that branch. These are the reasons for the association of the chapters on The Scope and Aims of Physical Anthropology with those on its History.

Thus, his integration of history with theory (as found in "The Scope and Aims") became a key work in 1919.

American physical anthropology was in many respects Hrdlicka's brainchild. Its early development was largely due to his guidance and directions. Hrdlicka defined the

discipline "in the briefest form as the study of man's variation" (1919b:8). Dealing with man's anatomical and physiological variation, physical anthropology dealt with "the causes and ways of human evolution, and with the development, transmission, classification, effects and tendencies of man's bodily and functional differences" (1919b:8). In what might be called a one-paragraph apology of the discipline, Hrdlicka defined physical anthropology as an integration of anatomy's and physiology's structure-function orientation toward man, with biology's structure-function orientation toward relating the various species one with the other--both extended to embrace time, race, society and pathology.

Justifying the present (1919) status of physical anthropology on the basis of past accomplishments, and using the same accomplishments as a theoretical basis for the jump into the future, Hrdlicka summarized the state of the science. Perhaps an extended quotation is in order, in as much as it expresses Hrdlicka's concept of what physical anthropology is all about.

> ...Physical Anthropology has already accomplished considerable useful work. It has established a system of precise measurements of man and his remains, and furnished the needed instruments; it has directly advanced general anatomy, particularly that of the skeletal system and the brain of man, and other primates, and has contributed to zoology, general biology, and other natural sciences; it likewise has established the physical

> knowledge of the races and many of their subdivisions. Through its activities it has also accelerated the advance of its sister branches, ethnology and archeology. It has given a marked impetus to search for the remains of early man and inspired thorough critical accounts of the physical characteristics of the finds made. It has actuated and to a large extent carried out the study of the development of man from his inception onward; it has brought about physical investigation and through it an enhancement of our knowledge of school children as well as of advanced pupils, of recruits, and of the criminal and other defective, delinquent, or dependent classes; and has led directly to practical systems of identification of criminals. It has participated in and promoted studies in human heredity, degeneration, and hybridity; it has increased our knowledge of the functions and pathology of the human body, and especially of the brain; it has furthered the gathering of vital statistics; and it has already taken steps toward aiding other branches in determining, on the basis of acquired knowledge, ways toward safeguarding and improving the human race. This outline is necessarily condensed, yet it will indicate in a measure that Physical Anthropology, notwithstanding the many and serious obstacles in its path, has already well justified its separate existence and the decrees by which the French Government pronounced it, in 1864 and again in 1889, a science of public utility (1919b:15-16).

Beyond this summary, Hrdlicka covered in some detail the aims and future of the discipline, embracing at once the meaning and relevance of physical anthropology. His work remains a landmark in this respect.

Secondary only to the myriad of disagreements, discussions, explanations and justifications attendant to the emergence and early development of the discipline, the principal theoretical dispute in American physical anthropology concerns the divergence between description-oriented and

problem-oriented scholars. The sometimes subtle, sometimes obvious struggle has been noted repeatedly throughout the present manuscript. The development and character of the question will be examined in greater detail at this juncture.

Within an academic climate peopled by physicians, anatomists and biologists, as well as physical anthropologists--a group inclined toward scientific measurement, description and comparison--there arose in the third and fourth decades of this century a trend toward problem analysis. It was becoming increasingly apparent, to the problem-oriented group, that the description and measurement of gross morphological characteristics, even when enhanced with mathematical and statistical analyses, was unsatisfactory procedure for the attainment of understanding. The actual desire to understand mechanisms relating to morphological differences characterizes in part the new movement. That questions such as "how much time would be required, biologically and evolutionarily, for the distribution of peoples from Alaska to Tierra del Fuego?" "how can we account for the pronounced physical variation among the various New World peoples?" and "can analysis of blood groups and of genetic traits shed light on physical variation?" were being asked illustrates a new line of anthropological thinking.

Before embarking on a survey of the literature pertaining to this development, however, remarks on the natural evolution of any science are in order. It will be seen in the immediately following pages that a specific temporal and constitutional division in American physical anthropology is sometimes invoked. Even today some anthropologists refer to the "before and after" aspects of the so-called "new" physical anthropology. Interpreting the state of the science as bordering on stagnation and academic decline, they hail the avenue of new and challenging vistas. When description and measurement failed to answer the questions which had so long been the special province of physical anthropology, they say, progressive physical anthropologists oriented themselves toward study which was to take them far deeper into mechanisms and functions of human biology. It should be remembered, however, that as a general rule all sciences proceed first with observation, description and measurement, and that theoretical and interpretative questions follow in time. For an example from physical anthropology, if human variation is the central interest initial phases of research are directed toward documentation of variation--through visual, mensurative and descriptive means. In due time, when the accumulation of grounded fact permits, evaluative questions are asked--questions such as "how?" "when?" "why?" and "through what

mechanisms?" In this respect there is a typical or natural evolution of a science, through natural and logical steps. Hence the introduction of new phases of research may be viewed as expected developments, rather than revolutionary upheavals. Let us examine, through the study of several selected papers, steps in the development of the trend in question.

Ashley Montagu, in a 1941 paper outlining the relationship between physical anthropology and anatomy and the most advantageous course of study of physical anthropology students, expressed several attitudes and beliefs which relate to the trend. He pointed out that the original and continuing symbiotic relationship between the two sciences is, by definition of the sciences, a permanent one. Anatomy (or morphology) studies the structure and function of organisms "within the matrix of very different and very variable environments, which... exercise a conditioning or modifying effect upon the morphology of the individual and of the group" (Montagu 1941:261). It is the latter factor which links the two disciplines, and anthropology takes up with the caliper where anatomy leaves off with the scalpel. Montagu's first point is that when one takes up the caliper it is poor procedure to completely dispense with the scalpel. In other words, Montagu felt that some anthropologists have made the

mistake of placing all their stock in measurement, to the exclusion of the functional and structural facets of anatomy. Montagu continues,

> Such men, however competent they may be at their work, can rarely succeed in becoming anything more than good technicians. At best they can accurately record end-effects, but they can never have any real understanding of the manner in which those end-effects are brought about.

His assessment might be qualified to read, "unless the technicians extend their searches beyond their techniques." We may also question whether in fact many scholars forgot the scalpel in favor of the calipers.

Montagu fully realized that the technique of measurement--as well as that of statistics, which he also assessed--is one of the most valuable and indispensable pursuits of the anthropologist. The problem is found to arise whenever technicians attempt, on the basis of their measurements and without further qualification, to predict underlying morphological phenomena. This practice, he says, can only serve to heap disrepute upon physical anthropology. The practice of offering unqualified and unconsidered judgments of an anatomical and functional nature is, in fact, only a step beyond offering measurements as an end in themselves, together with the tacit assumption that a problem has been solved.

In his call for a more practical and useful education for physical anthropologists--a course of study grounded in anatomy, biology and genetics--Montagu further voiced the need for understanding function and process (in short, the working aspects of a problem as opposed to the outward manifestations of that problem). "Science," he says, "is not descriptive, but analytic and integrative, and no man can be an analytic and integrative physical anthropologist without first being a morphologist" (1941:264). Drawing upon the analogy of learning an alphabet before proceeding to reading and writing, he characterizes morphology as the alphabet of physical anthropology. Reading and writing would, then, be analogous to the study of growth and development in the human species (1941:264,267). It is certain that if students of physical anthropology are not firmly grounded in anatomical and biological sciences, their potential for contributing usefully to the discipline is diminished.

Sherwood Washburn and S. R. Detwiler--the latter a skilled anatomist--continued, in a sense, the quietly ongoing argument for problem-orientation in their 1943 "experiment bearing on the problems of physical anthropology." Defining the science as 'concerned with the evolution of mankind and with the variation among the groups of living men," and finding that description and classification are the major

activities of physical anthropologists, they move through the assumption that problems are solved through flawless description and analysis, to the conclusion that not "even perfection in description will necessarily lead to correct explanations" (Washburn and Detwiler 1943:171). They point out that since the classic problems of anthropology have not yet been solved, the practice of explaining theories with purely descriptive data apparently is not workable.

Through illustrating how anatomy overcame the stumbling block of accepting description as a final answer, by branching out into experimental fields, they implied that physical anthropologists could as easily move into experimentation. For although "Physical anthropology is a comparative science... Many of these old controversies can be settled by the use of experimental methods" (1943:188). Experimentation however they explain as no easy task. It requires creativity and a willingness to continue searching. For while much may be learned through comparing the findings of one science with those of another science, experimentation must be adapted to fit the situation and needs at hand. Hence, "If anthropological theories are to be tested, experiments will have to be designed to fit anthropological situations" (1943:177). Washburn and Detwiler's proposal for anthropology was suggestive and challenging. Their purpose was not only to

stimulate new research procedures and priorities, but also to generate new ways of approaching a growing science.

W. W. Howells, one of the foremost general anthropologists in the United States, added a sobering assessment of the roles and goals of physical anthropology in a 1943 article titled "Physical anthropology as a technique." He suggested that although the discipline is usually defined in terms of the evolution of man and races, embracing paleontology, zoology, anatomy, physiology, prehistory and ethnology, "the problem of the application and direction of physical anthropology is... giving concern to its students, (which) may be seen in various signs, such as the current interest in methods of measurement" (Howells 1943:355).

In seeking to answer the oft-asked question "Why do you measure skulls?" Howells offers that the reason lies in the basic scientific procedure of physical anthropologists. Measuring takes the scientist from a reliance on intuition to a grounding in fact. And it is from the grounding in measurement that the search for knowledge and understanding is pursued. Methods of measuring, that is, were simply ways of numerically describing the subjects, and as these means became refined measurement was seen less as a way to describe an individual or a single group and more a way to compare individuals or populations. The eventual result was a system

for comparatively distinguishing populations, in a controlled laboratory setting. This accomplishment is undoubtedly "one of the requisites of an experimental science" (1943:357). At this point Howells has answered why we measure skulls. Continuing, he graciously overlooks the thoughtless scientists who have scorned their colleagues as "caliper anthropologists," "probable error anthropologists," or "cadaver anthropologists;" he sees that there is room in the discipline for all interests, and yet he finds no substitute for measurement and statistical analysis. For while some anthropologists may have overlooked the significance of measurement and statistics, these techniques have never been seriously questioned as appropriate to the effective study of human biology. So we now ask: Measurement, description and statistical analysis are indispensable techniques, but where do we go from there?

In further analyzing the relationships between physical anthropology, and genetics and biometrics, Howells concludes that as a whole, physical anthropologists have at their disposal "a sound and useful technique, but one which has not by any means been completely explored" (1943:359). Howells is then pointing explicitly to the future, urging physical anthropologists to go beyond techniques and the present, to "wider uses of our general method." He envisions physical

anthropology, as a technique, contributing ultimately to the fuller understanding of man. He suggests, for example, that population variation and differentiation--complex and involved questions which have been inadequately investigated--should be approached on the basis of existing techniques. He suggests, in addition, that a general anthropometric study should be designed, for no nation--least of all the United States--has a true idea of its internal physical composition. Then we might direct ourselves to studying the forces which affect physical form, as well as the significances of differences which exist with respect to geographical region and social class. Speculating that it might be valuable to know the distribution of head size, geographically and socially--information which is almost totally lacking--Howells wonders whether head and body size indicate any factors of constitutional vigor, whether there is a relationship between increased stature and better health conditions. In summary, Howells was directing the capabilities of the physical anthropologist toward the study of social questions, bridging the gap between physical anthropology on the one hand, and social anthropology and sociology on the other.

A basic aspect of the growing re-orientation on the part of some anthropologists was the increased reliance on genetic principles and theory to solve anthropological problems. In

1950 William C. Boyd, a biologist and geneticist who has made numerous contributions to physical anthropology, presented a volume on Genetics and the Races of Man, in which he outlined the potential of genetics for understanding the processes and results of human evolution and the origin and character of races. Joseph B. Birdsell, in a 1951 review of the book, at once summarized Boyd's position in the matter, and gave a more general assessment of the widening rift in the discipline. Of the work he says, "Its minor flaws result from the author's enthusiastic but presently premature conviction that a genetical approach can answer all of today's questions concerning human evolution" (Birdsell 1951:223). For Boyd and other scientists, newly invigorated and directed by the findings of genetics and the obvious future of this technique, were increasingly impatient with other scientists who were not so ready to dispense with comparative morphology. Thus, while some were perhaps tardy in embracing the new technique--and with good reason in some cases, it will be shown--others were led by their enthusiasm to undervalue the contributions of the earlier techniques, namely comparative morphology through measurement and description.

Herluf H. Strandskov and Sherwood L. Washburn published an editorial, "Genetics and physical anthropology," in the Journal in 1951. They pointed out the recent trend to consider

genetics and physical anthropology as "necessarily opposed," based on the denial by some of the utility of anthropological methods for solving racial questions, as well as the feeling by others that the genetic concept of race was unrealistic. They held however "that the basic idea of race is the same no matter whether races are described in terms of anatomical traits or gene frequencies," denying further that there is a basic "contradiction or conflict in the use of both genotype and phenotype in the description of races" (Strandskov and Washburn 1951:261). It is seen here--and in Boyd's and Birdsell's comments, incidentally--that the more basic disagreement on anthropological approaches was focused on the critical issue of race. They pointed out further that to study human races without utilizing available genetic information was to needlessly limit oneself, while disregard for anatomical information was equally limiting.

The interrelationship between morphology and genetics, between phenotype and genotype, was also stressed in their paper. That is, while completeness of a study requires the description of all phenotypic variations, these phenotypic traits are further explained and understood through the knowledge of genetics. In no sense then can it be said that Strandskov and Washburn advocate the abandonment of comparative morphology in favor of genetics. Witness their conclusion:

> Human genetics and human anatomy supplement each other, and both are necessary for the understanding of race. As knowledge of human genetics advances, the apparent barriers between genetics and anatomy should disappear (1951:261-262).

The comments in this particular editorial should be noted with due attention and seriousness, for they serve to temper and explain subsequent statements by Washburn.

T. D. Stewart commented on "Objectivity in race classifications," again in 1951. Referring to Strandskov and Washburn's editorial, he said, "The point that they make about the inseparability of anatomical traits and gene frequencies in matters of race is precisely what I have in mind. Any genetic concept of race that ignores anatomical traits is unrealistic" (Stewart 1951a:471). He expresses in this communication, as he has in numerous other publications and in private communications, that the issue of comparative morphology (or traditional anthropology) versus genetics (a new anthropology) is a misunderstood and somewhat untimely argument. Feeling a continuity in the natural evolution and emergence of American physical anthropology, he has deplored the abrupt and irreverant break with the past, a rejection which in large part has undervalued the significance and contributions of traditional anthropometric methods and techniques. While he has never questioned the value of new and innovative techniques such as

human genetics, in other words, he has maintained that a sensible balance in anthropological thought requires continuity from the past to the present.

Stewart has questioned in particular, however, geneticists' claims of higher objectivity. Assuming that the basic data of the science is relative qualitatively, "it is debatable whether the genetic classification of races rests upon a higher degree of objectivity than do the morphological classifications" (1951:471). He was skeptical of the statistical manipulation of figures from the basic data, relating to gene frequencies. He objected also to the single-directedness of most proponents of a genetical revolution in anthropology. The "special pleaders," for example, sometimes ignore alternate interpretations, in their quest for a genetic answer to anthropological and racial questions. Thus, while "the applications of genetics to anthropology do not need special pleading (since) their value is clearly evident," it is difficult to proceed from phenotype, which is easily discerned, to genotype, which is understood only with considerably difficulty. With this point made, he concludes that what Boyd and others have called a "revolution" might better be characterized as evolution (Stewart 1951:472).

In the following year Birdsell also commented "On various levels of objectivity in genetical anthropology" (1952). He

began by agreeing generally with Stewart's position that the genetical approach to solving taxonomic problems in race does indeed involve non-objective aspects. He disagreed however on the locus of subjectivity. Calling for sober evaluation of the over-simplifications generated by the genetical approach, he pointed out that sampling in anthropological research involves several factors. Among the variables are phenotypic characters, cultural considerations, geographical influences, other environmental variables (which test for adaptedness in human populations), and isogenic patterns (which may suggest new areas for sampling). It is clear then that phenotype--the character to which traditional anthropology has catered--is but one of several relevant factors. Birdsell proceeded however to the rather curious position that phenotype is not essential to the genetical approach, that it is theoretically feasible to eliminate phenotype. It is clear from this statement that Birdsell had a minimal respect for the future of the traditional technique of comparative morphology. For while he concurred with Strandskov and Washburn that genetics and morphology supplement one another, that both are necessary for a full understanding of race, and that "metrical measurements may continue to provide important information for studies both in racial taxonomy and the processes of race formation,"

he clearly called for reform of the old techniques. In other words he says that morphological characteristics and metrical measurements

> are in need of reanalyses designed to provide insight as to the effect of functional and other environmental influences upon their expression. This would not be a continuation of the classical approach to race, but rather a salvaging of some phenotypic characters formerly utilized (1952:358, italics added).

Birdsell continued to define other subjectivities in the genetical approach. The limiting boundaries of a population, for example, are not generally objectively defined. In techniques which must yield results with high accuracy, absolute objectivity has not been obtained. Bias is found in mathematical techniques for estimating gene frequencies from phenotypic frequencies. The ten loci which are used for determining gene frequencies in a human population are statistically inadequate. It is noted however that the subjectivities of the genetical approach do not lend objectivity to the classic approach, even though the former approach is subjective on a higher level. Hence, only a beginning in genetics has been forged (1952:359-360).

From this stance Birdsell proceeded to level even more stringent assessments on the classical approach. Population genetics is held to be a progressive step forward from the

classical approach, "with its emphasis on the individual rather than the population, its preoccupation with typologies, its use of unanalyzed phenotypic traits, and its very paucity of terms for the identification of the basic forces of evolution..." (1952:362).

With reference to Birdsell's statements, and subsequent reactions and events, several questions may be probed. First, it is questionable whether anthropologists working with classical methods had concentrated on the individual rather than the population, unless one chooses to journey back to the nineteenth century for examples. For the researches of Boas, Hrdlicka, Hooton, Krogman, Todd, Stewart and many others show clearly that population variation had long been the object of study. Furthermore, the priorities of the genetical approach--priorities which were imposed upon physical anthropology, with little question or evaluation--are at least worthy of attention. For while population genetics calls unconditionally for population as opposed to individual studies, it is unrealistic to imply that studies of the individual hold no more relevance for physical anthropology. Yet this is precisely what Birdsell, Boyd and other geneticicists have done.

Second, why is it said that the classical approach has been preoccupied with typologies? Before the advent of

human genetics, had anthropologists actually been preoccupied with constructing typologies, or had they rather, through their various researches, managed to construct some classifications and make some sense of a confusion of data? Are not classification and systematization among the first and most important enterprises of a science? It should be remembered, in this context, that physical anthropology in America is a very young science, still suffering many growing pains. And it is difficult to follow Birdsell's claim (when he disagrees with Stewart) that progress from the classical approach to the genetical approach looms "larger than gradual evolution," that "it stands at least as quantum evolution, and perhaps as a revolution" (1952:362). While this matter is certainly one for individual appraisal, bearing only indirectly on the independent reality of affairs, Birdsell's position appears to be unsubstantiated. In continuing to deal with the second point, Birdsell may be queried on the future and direction of the genetical approach. That is, can this approach present its findings, and promote understanding of race and other problems in human biology, without constructing typologies? And do we even wish to dispense with typological constructs, since they seem to be valuable means of working with and referring to data?

Third, the "use of unanalyzed phenotypic traits" is a puzzlement. If "unanalyzed" is meant with reference to genetic and biological investigation, then of course phenotypic traits have been unanalyzed. It is beyond question however that phenotypic traits had for a number of decades been subjected to critical and continual analysis in so far as available means permitted. That the data had not been processed with respect to genetical knowledge and procedure--since the latter was only in nascent stages of usefulness--is no basis for disregarding accomplishments which had accrued to 1952.

Fourth, when Birdsell referred to the "paucity of terms for the identification of the basic forces of evolution," he seems to have underestimated the taxonomic and scientific accomplishments of pre-1950 physical anthropologists. If he had referred to the lack of understanding of a great many basic forces and processes of evolution, he would have been partially correct, although still partially unfair in implying that physical anthropologists had not kept abreast of evolutionary enlightenments. But to question the evolutionary terminology of physical anthropologists is a move which remains unclear.

These objections--questions and statements--are not offered as a defense of comparative morphology and metrical

measurement, nor as offense to human genetics. They are however directed to Birdsell and his evaluation of classical anthropological approaches. Elaboration has been indulged at this point in order to suggest that a great range of considerations, many of which have not always been acknowledged, are relevant to the great mid-century debate in American physical anthropology.

Thus while the trend toward problem orientation had reached a forte during the 1940's, and had visibly centered on racial anthropology and the introduction of genetic anthropology in the late 1940's and early 1950's, a prelude to the climax was evidenced in 1950 at the Sixth Annual Summer Seminar in Physical Anthropology, hosted by the Wenner-Gren Foundation in New York. This seminar, organized and directed by Washburn, and reported by Bernice A. Kaplan (1951), focused on the definition of physical anthropology. It was pointed out by some of the members that concern for techniques had in the past been a preoccupation for physical anthropologists, whereas the time was ripening for more emphasis on problems and the understanding of processes. The trend reached a climax and triggered a new set of arguments and discussions with the publication in 1951 of Washburn's "The new physical anthropology." This article marks both a turning point in thought regarding the purposes and ways of physical anthropology,

and a polarization of sympathies and interests. Washburn's paper seems to have evoked a great deal of emotion--probably far beyond the degree of personal involvement which Washburn may even have suspected. While in some ways the article triggered emotions and controversy, in another sense it only accentuated, mirrored, and verbalized the disagreement. Unfortunately, Washburn's assessments and pleas have been repeatedly misunderstood and misrepresented. A careful and thoughtful reading of the paper, as well as its companion article, "The strategy of physical anthropology" (Washburn 1953), is the only probable means of understanding just what Washburn said.

Washburn set the stage for his 1951 paper by noting that paleontology and systematics have experienced a change in orientation, namely, from sorting the results of evolution to understanding the process and mechanisms of evolutionary change. Physical anthropology is then shown to be experiencing a similar reorientation. That is, in the past (before 1950) physical anthropology had been conceived and accepted as a technique--as, for example, the measurement of external forms with calipers. Measurement, classification and correlation were the anthropologist's tools and occupation. Washburn continues, "anthropology was characterized by theories, or rather by a group of attitudes and assumptions" (1951:298).

On the other hand, he noted there had been little development of theory in physical anthropology itself.

"Physical anthropology should change," Washburn said. (And here began the controversy.) The new physical anthropology was conceived to be "an area of interest," directed, through use of the most efficient techniques, to understanding the very processes of evolution and human variation. It is a return to Darwinism--to selection, mutation, drift, migrations--but with an added element. That element: genetics. In the meantime, the idea was to be a complete reorganization of and reorientation toward anthropological method and technique, rather than only "the adoption of a little genetic terminology."

Washburn's guiding principle was "that the major force in evolution is selection of functional complexes." For factoring out functional complexes, four methods were presented: comparison and evolution, development, variability, and experiment. The goal, then, was to go beyond making the simple statement that a trait is present or absent, to understanding the actual conditions under which it might occur. In short, try to understand process. For if only taxonomic aids are attained, then no means is provided for going beyond taxonomy to understanding. It seems more advantageous to leave research open-ended, responsive to a varied array of questions, than to

answer point-blank a simple question and never go beyond that answer to its reasons. While Birdsell seems to have placed a low premium on taxonomy and typology, Washburn did not proceed to evaluate the significance and importance of taxonomy. We might note, for example, that ideally taxonomy seeks to do more than merely classify. It expresses relationships and requires in its own right some understanding of function and process.

Nevertheless, having suggested the undertaking, Washburn provided a three-step procedure for its accomplishment. He suggested that first the complex should be diagnosed, then methods for description of variations in the complex should be developed, and finally the genetic background of the variations should be sought. Such a method of study, he claims, is of more than philosophical importance. The understanding of functional anatomy may lead to advances in genetics, anatomy, medicine, and of course, physical anthropology.

Washburn's second paper on the new physical anthropology, "The strategy of physical anthropology" (1953), was in essence a continuation and clarification of the first. The state of affairs at that time was analyzed with regard to purpose, theory, technique and interpretation. He says in his conclusions, "The strategy of the traditional descriptive investigations has been contrasted with the developing analytic strategy,

with its emphasis on theory, process, and experiment." He gives here a clue to his personal concept of the significance of the new physical anthropology: "The new strategy does not solve problems, but it suggests a different way of approaching them." The new strategy, in other words, is more integrating in nature than is the old. It allows for a more detailed interrelationship between the branches of anthropology, based on an understanding of functional dynamics rather than morphological description. The new strategy was held to be necessary because the discipline had passed from its initial descriptive phase, wherein it needed a descriptive strategy, to a second phase, analytic in nature.

Distinguishing the old from the new physical anthropology, following Washburn's treatment, we find the purpose of the old was primarily classification, correlation, and description of differences; the purpose of the new is to understand process and the cause of differences. A larger step is taken by the new, in other words, toward the ultimate anthropological goal. Classifications, which were sometimes ends in themselves, now merely present new problems. The idea is not to outgrow or discard classification, but rather to assign it a preliminary, "rough-mapping" role, and go from there. Theory in the old physical anthropology was scant and relatively unimportant, he says. In the new it is critical; consistent,

experimentally verified hypotheses are sought. This history is understandable however, and it might have been predicted by an astute observer. It would seem, further, that the discrepancy between old and new is not quite so pronounced as Washburn suggested. Theory is not generally considered important in the first stages of a science. As complexity increases and knowledge expands, however, theory becomes a pressing necessity. Hence we observe again that physical anthropology, despite all the discussions of the period, was passing through a natural stage of development. Had Washburn and others made this point clearer, rather than minimizing it, many disagreements might have been avoided. On the other hand, the period of anthropological self-analysis would also have been missed.

After presenting a summary of theoretical thought, with respect to the various anthropological investigations taking place, Washburn makes a plea for agreement on four points. First, he calls for "a consistent, proved theoretical framework" for physical anthropology; this is the same point for which the present manuscript pleads. Second, evolutionary and genetic theories which are available should be applied to problems in human evolution. Third, untenable concepts should be discarded, and fourth, the time of transition should be welcomed, attended with great differences in opinion.

The scholarly atmosphere requested by these points would indeed enhance the discipline.

Technique, in the old physical anthropology, depended about 80 per cent upon anthropometry and morphological comparison, in Washburn's estimation. In the new, measurement accounts for only about 20 per cent of research. The remainder consists of specially developed techniques, appropriate to the research project at hand. It should be remembered that "techniques... exist only to solve problems and not as ends in themselves." In other words, some investigators have perhaps lost sight of the ultimate goal and have accepted, in the meantime, a technique as being a final reality. Interpretation consisted of speculation under the framework of the old physical anthropology. In the new, however, objective proof of the hypotheses is sought. The new takes up where the old left off.

In retrospect, and with specific reference to Washburn's theories, it seems that Washburn overstated his case in some respects. This may well have been his tactic. For it is recognized by some that by creating overstatement (despite the risk of simultaneously producing alienation in the reading public), one can bring the reader to a midpoint in thought--the point to which it was originally desired to direct his mind and conviction. Whether this indeed happened with

respect to Washburn's efforts is unanswerable. The significant point is that for the most part Washburn was moving in a philosophically sound direction. He recognized that the discipline was growing and that theory and method would, of necessity, change. So he was more a heralder of the change than a precipitator. It is regrettable that in the process of presenting his ideas he alienated many of his colleagues. Had he stressed more the linkage between the old and the new--an evolutionary relationship--as did Stewart (1951b, and personal communications with the author), fewer might have been antagonized. A less stringent reassessment of anthropometry and its place in anthropology might have been more compatible with some anthropologists. But this was not Washburn's tactic. How he handled his insight is a pointless question to argue. That changes occurred in physical anthropology is the focus of importance.

Washburn has recently presented another historico-theoretical work, "One hundred years of biological anthropology," in the form of a chapter in J. O. Brew's One Hundred Years of Anthropology (1968). This paper illustrates, in various unstated ways, that the trend of the third, fourth and fifth decades of this century has been partially accomplished. For while it is a challenge to tackle new research frontiers, yet it presupposes that problem-oriented research

is a widespread reality. By way of illustrating, through traditional and current physical anthropology textbooks, that those scientists called physical anthropologists are actually teaching much broader ranges of information than their own researches carry them, he points out that any available information is incorporated, say, in evolutionary study. Biological anthropology he defines even broader--"the biological element in any anthropological problem" (Washburn 1968:97, footnote 1). His discussion, however, is limited to the origin of man.

Washburn's essential point seems to be that today--after the development of anthropology and of its constituent branches, and of the separate history and development of each branch--we find we have completed a circle. For while physical or biological anthropology emerged as a blending and integration of other sciences, to the accomplishment of a specific study of man, emerging as a stable discipline--and the American branch later accepted a position in the academic framework with social and cultural anthropology, linguistics, and archaeology--today the discipline cannot be considered to exist and function independently. Although biological anthropology functioned as an interdisciplinary approach at its inception, its role in interdisciplinary study today is crucial. In short, physical anthropology is today an integral part of a much greater whole.

With view toward today's role for physical anthropology, Washburn has made a number of suggestions--ideas which may be interpreted by some anthropologists as embodying a number of accusations. A summary of his suggestions reveals several major points. He feels that manipulating types in order to reconstruct evolutionary history is no longer acceptable. Nor, in his estimation, is the study of nonadaptive characters appropriate to physical anthropology interests. He stresses also that orthogenesis must no longer be studied in lieu of the actual setting of evolutionary process. For he focuses on a central evolutionary problem which, he says, "is behavior and, in the case of man, the principal adaptive mechanisms are social and are dependent on the brain and the behaviors that it makes possible, on language, tradition, complex skills, and social change" (1968:114-115). No one scientist, nor any other scholar, can handle problems of such complexity, of course. Synthetic theories and synthetic ways of teaching are needed. Behavior and biology must be integrated, for one does not exist independently of the other. They are interwoven in a complex mass of life.

This complexity, as we study it, is both enhanced and obfuscated by the history of anthropological study and, indeed, the whole history of the study of man. Among the complicating factors, as Washburn presents them, are the gradual emergence

of a theoretical structure; the appearance and assessment of the fossil record; the concept of time, and its many ramifications and secrets; and the concept of space as it relates to human evolution and adaptation. Each of these broad areas of conceptualization is in itself a universe of investigation. With respect to time, for example, there is biological time (circadian rhythms), social time (as, three generations), historical time, and geologic time (which is almost beyond comprehension). Each of the aspects of time has relevance for both man's life and evolution, and the biological anthropologist's assessment of where man has been for the past few million years. The integration of such seemingly remote or isolated factors with all other factors, into the grand scheme of life, is what anthropology is all about.

Buettner-Janusch has recently expressed his views on the nature and future of physical anthropology, in an article by that title (1969). He confirms that since the introduction of the "new physical anthropology" there have indeed been changes. And he addresses himself to the need for a continuing watch over the avenues of anthropological research. For physical anthropology, he says, does not actually have a secure home in American academic structure. This he attributes to the "dilettante, humanist strain within anthropology." Hence, it is to the interests of anthropology as a whole that the physical branch of the science be a firm bastion of scientism.

The vast amount of "garbage" floating on the stream of physical anthropology, as he puts it, is the source of great dismay to Buettner-Janusch. Envisioning the broader subject of physical anthropology as the paleontology and neontology of the entire order Primate, he narrows the central theme to the evolution of the human species and its culture. To this end molecular biology should be of ultimate importance to physical anthropology. Taxonomic and phylogenetic theory, as well as the possible calculation of rates of evolution, may be better understood through the principles of molecular biology, than through more traditional anthropological research techniques.

Buettner-Janusch acknowledges that new methods of study often produce contradictory data and results. The tactic is to discard neither, in indiscriminate fashion, but rather to be aware that the problems are indeed often more complex than first analysis indicates. Carelessness, then, is a barnacle to physical anthropology. Another barnacle is the historical approach in teaching physical anthropology and human evolution. Yet another--in Buettner-Janusch's analysis--is traditional osteological study. His stringent comments, reproduced in the anthropometry section of Chapter VI of this paper, are appropriate here.

He goes on to say that physical anthropologists of today must be trained in biology, as well as in traditional anthropology. In general, a new kind of physical anthropologist must be produced--one who, presumably, will be able to meet the new anthropological problem areas as Buettner-Janusch has envisioned them. For paleoanthropologists there are the tasks of clarifying the origin of the primates, defining and clarifying the origin of the hominids, tracing affinities of Eocene ancestors and modern primate lineages, and a re-evaluation of Pleistocene hominids. In general, contribution of the genetic approach and the significance of variability must be taken into account.

Neontology, Buettner-Janusch says, must be investigated now in terms of variation, population and molecular genetics, and behavior. Racial studies have also changed. The significance of bumps on the head or shape of the nose is now seen to be minimal. Fruitful study does not lie in pursuing such questions. Growth and development, and variation in growth patterns are, for example, questions now pertinent to race study.

Environmental adaptation is a study most appropriate for physical anthropological investigation. The ability of peoples the world over to adapt their bodies and ways of living to cold and heat, altitude, solar radiation, and so forth, is

noted generally but scarcely understood at all. An understanding of the principles, functions and results of population genetics is another need to which anthropologists should now address themselves. In a sense, environmental adaptation and population genetics studies have replaced traditional race studies. The implication here is that a new kind of question is being asked, or must be asked--that of processes and functions, rather than simply description of observations. The difference is to understand and know, rather than only see.

Buettner-Janusch does not think, then, that physical anthropologists can usefully spend their time classifying races, or otherwise occupy themselves in defining Negroid, Caucasoid, etc. He goes so far as to say that these are not valid scientific terms. Such questions in general are no longer pertinent. It is even of little importance, he says, to classify Homo sapiens below the species level. It will become clear in Chapter VI, however, that few anthropologists find acceptable this rejection of race study. From the beginning of the physical anthropological approach to the study of man, race has been a question of primary importance. In the 1950's (as has been shown in this chapter, and will be amplified in Chapter VI), feelings about anthropological contributions to race and the understanding of race differences

reached a critical pitch. Race has offered to physical anthropology and its students an avenue for contributing to the solution of world problems and humanitarian understanding. It would seem not only sad but also unjustifiable to dispense with race now, when possibilities for solution and contribution are ever-increasing.

It has been suggested previously in this manuscript that what some called a division in theoretical anthropology, a transition from "old" to "new" anthropology, might better be described as a significant phase in the evolution of the science. It is a factor of historical accident, in other words, that physical anthropologists spent many years measuring skeletons. They were accumulating, recording and classifying basic materials, in order that their data might eventually be used to intelligently form theoretical constructs and to define realistic problem areas. It has been an unfortunate implication on the part of some anthropologists that other and earlier anthropologists spent their time measuring skeletons, that they were simplistically content with this occupation, and that they had no visions of integrative, analytic and interpretive studies. It will be seen in Chapter VI, for example, that Buettner-Janusch has leveled just such a pointed attack. Other anthropologists, while perhaps not subscribing to this exclusive view, have failed to sufficiently

clarify their conception of the integration of old and new anthropology.

It is interesting, in another light, that physical anthropology has from time to time been chided or berated for a somewhat tardy acceptance of human genetics--a system of knowledge and a research technique frequently heralded as the salvation of a sagging discipline. For physical anthropology might alternately have been complimented on its very capacity for drawing upon related fields for both data and research tools. Physical anthropology, and particularly the American branch of that science, is unique in this respect. Its entire development and evolution reflect a series of integrations and adaptations. In the role of a bridging discipline, linking the various natural sciences--paleontology, biology, geology--with the social sciences, in its attempt to describe and understand the bio-social nature of man, physical anthropology has always selected its data and research techniques largely from the storehouses of other disciplines. Consequently there are many tailor-made techniques used in the discipline. Not only have borrowed techniques been modified and adapted, but specific problems encountered by physical anthropologists have demanded the generation of new techniques also. The unique character of physical anthropology, and of its goals, endeavors and procedures, have resulted in the

development of new avenues of research, oftentimes unconventional avenues. Thus while even today anthropologists would like to deal with genotypes, and are continually searching for new data and research techniques, borrowing from other disciplines (see, for example, the profusion of genetical studies in the American Journal of Physical Anthropology from 1950 through 1970), yet they must often be satisfied with phenotypes. While physical anthropologists have continued to search for better methods and techniques, and can proceed only so far as current knowledge will allow, the integrative nature of the discipline draws us on.

Prerequisite three for the establishment and justification of a scientific discipline, as presented in Chapter I, states that

> There must be a theoretical framework within the discipline, for the incorporation of goals, methods, procedures, and so forth. A basic theoretical structure is needed, within which all research and study may fit. In this respect, an apology of the discipline is also appropriate.

This chapter, in conjunction with the foregoing chapters, has demonstrated that there is in fact a substantial body of theory in American physical anthropology. The theoretical framework may be reviewed on several levels of abstraction. The overall integrating structure, based on major trends

affecting all American physical anthropologists, has been presented here. Chapter VI will be partially concerned with theoretical implications of a more discrete sort, relating to the development of specific research areas in physical anthropology. The need for an integration of anthropological theory is particularly noted.

CHAPTER V

THE PHYSICAL DATA

The fourth prerequisite in the analysis and justification of an academic discipline states that the discipline must have at its disposal either a body of physical specimens or a specific method and category of thought. In the case of natural sciences, whose relationship to the social sciences physical anthropologists strive to bridge, there must be a body of specimens which serve both to stimulate research and to become the subject of research. It is axiomatic that the physical specimens are specific and peculiar to the discipline in question. If this condition cannot be met, the characteristic orientation of scholars to the specimens assumes importance.

It has always been assumed, in an America of bountiful resources and limitless possibilities, that physical anthropologists have plenty of subject matter at their disposal--more, at least, than they could ever hope to process. The range of resources available to physical anthropologists is indeed broad, and each area is richly productive. But such abundance is not unlimited. At a rather early date American physical anthropologists became aware that the racially homogeneous American Indian populations would not forever remain unmixed with other races, and that they might in time become

extinct. And while today we see that rather than becoming extinct many Indian groups have begun steadily increasing in numbers, yet the original anthropological utility of the native populations--as a racially unmixed population living in an undisturbed environment--is today much less than originally. Actually, as early as the turn of the century Franz Boas was critically aware that valuable data was daily facing disintegration and re-emergence in other forms. He devoted a great part of his scientific life to the study of disappearing peoples. Thus while we sometimes lose ourselves in the wealth of data accumulated by Boas, Hrdlicka and many other pioneer anthropologists, we must still face the decision between pursuing new and perishing data and the never-completed processing and study of old data. Research personnel are far too limited in numbers and time to fully satisfy both needs, yet we can ignore neither.

We have a glimpse, then, of a number of problems and deficiencies that attend the study of physical anthropology in the New World. With this in mind, the very framework in which American physical anthropologists function bears further investigation. It has been suggested from time to time that American physical anthropology does not function as a discipline separate and distinct from its European counterparts. This view holds that physical anthropology, by

whatever name, is essentially homogeneous the world over, and that at whatever base and with whatever data, all physical anthropologists do essentially the same thing. Others, and particularly some Americans, feel that there is indeed a distinct character in the American branch. They look to history in order to explain the differences and how these differences came to be. The availability of specimens--what and how much there is to study, as opposed to how it is studied--is probably the factor which best defines each geographical group of physical anthropologists.

In Europe the study of early man and his prehistory is a long-standing science. The line of study developed, as we have already seen, from a natural interest in man's origins, and came to be known as "paleoanthropology." When fossilized hominid remains were discovered in various localities across the continent, physicians, anatomists and other physical and natural scientists fell natural heirs to their study. In due time, as the discipline became structured and formalized, the search for early man led Europeans into Asia and Africa. And by and large, Europeans held reins on the field of paleoanthropology, if for no other reason than because American scientists did not exert a substantial effort to enter the field. A notable exception is Hrdlicka, who maintained an engaging interest in hominid prehistory, traveling extensively in Europe, Asia, Africa and South America in order to review

and amass evidence. Several factors suggest the failure of American scientists to manifest an active and engaging interest in paleoanthropology. Primary perhaps was the relative youth of higher institutions of learning, coupled with the lack of an abundance of truly ancient fossils in the New World. Great geographical distance also served to keep Americans apart from Old World research and thereby precipitated the default. Paleoanthropology has emerged, then, as a distinctively Old World enterprise. Americans, when they elect to study fossil man--as have, for example, T. D. McCown, T. D. Stewart, C. S. Coon, Clark Howell, Hallam Movius, and various others--have had to go to the Old World and function in an Old World environment and academic structure, simply because this is where the fossils are.

The consequence of this whole course of events was an Old World claim on the study of the origin of man. By natural course this broad field of investigation was expanded and incorporated the entire evolution of man, the origin and diversification of races, the birth of culture and of agriculture, and the relationships of both fossil non-human primates and almost all extant non-human primates to Homo sapiens himself, with the obvious exceptions of some few specimens in the New World (which Europeans also helped to recover). In great dismay one may ask, then, "What is left for the American physical anthropologist?"

By contrast, we find that American physical anthropologists fell natural heirs to the study of the aboriginal populations of the Americas. (That the first scholars to pursue topics of physical anthropology in the New World were in fact Europeans with European backgrounds, does not detract from the present argument, for these scholars were breaking new ground, pursuing a new discipline in a new land, under new academic auspices.) The American Indian, from the moment of his discovery by Europeans, was a source of the most intense worldwide curiosity. We have seen how, through the efforts of such scholars as Warren, Morton, Wyman, Allen, Meigs and others, as well as through western expeditions, and finally, through the calculated foresight of the Boas and Hrdlicka generations, the American Indian was exploited. Living populations were utilized by American scientists as anthropological specimens to be studied, analyzed and documented as soon as possible. For even during the nineteenth century the more far-sighted mind could comprehend the ultimate fate of the American aborigine. The recovery of American Indian skeletal remains was given high priority, although it was clearly apparent that these data were less perishable, over a few decades, than were living populations. The logical extent of the anthropologist's desire would be to have every available skeleton properly housed in a laboratory box. And while this desire is

hardly feasible, the notion serves to illustrate the collecting and possessing orientation of early American physical anthropologists. It is also significant that here, as in the Old World, past and present, skeletal material has consistently been brought to light by construction and excavation enterprises.

Unfortunately, the recovery of human skeletal material, both in the Old and New Worlds, has not always been realized with proper scientific procedure. Insufficient knowledge of geological principles and unskilled excavation techniques contributed to this condition. Perusal of the U.S National Museum collections, for example, reveals hundreds of isolated skulls, many unidentified. It is lamentable, in many cases, that post-cranial material was not salvaged. The disposition to accumulate skulls is most apparent, by way of example, in Morton's efforts to build a cabinet of crania. Interests in race (the early ethnology) and phrenology dictated that skulls were the most important remains. Post-cranial material obviously was of insufficient import to merit retrieval. Problems relating to short-sighted procedure of a bygone day are exemplified by the author's difficulty, examining skulls at the U.S. National Museum, in finding a large sample of pathological skulls which were accompanied by post-cranial bones.

Nevertheless we have at our disposal today several large skeletal collections. The U.S. National Museum collection,

largely a product of Hrdlicka's single-handed efforts, is the foremost osteological collection in the New World. The thousands of individual specimens trace their origin not only to every locality of North America, but also to Middle and South America and most every other region of the world. The American Museum of Natural History in New York City also houses an extensive and valuable collection of human skeletons. Besides these major collections, most major state universities and a number of private universities maintain extensive skeletal collections. These collections are discussed in more detail in Chapter VI. American physical anthropologists have at their disposal, in addition, two major clinical collections. The Todd Collection, assembled by T. Wingate Todd at Western Reserve University, and the Terry Collection, assembled by Robert J. Terry at Washington University in St. Louis and currently housed in the U.S. National Museum, contain documented medical cases, the skeletons of which are in excellent condition. The primary shortcoming of these collections derives from uncertainty of accuracy on medical records, as well as the fact that the sample is skewed toward certain age and socio-economic brackets.

Non-human primate fossils have also been found in the New World. They have, however, appeared in rather small numbers and for the most part have been inadequately studied

and interpreted--inadequate when viewed in terms of interpreting their role in the greater evolutionary picture. We know little, for example, of the overall picture of morphology, ecology, physiology, distribution, etc. We do know that the geographical source of New World fossil primates is the northern half of South America, Central America, and as far north as Texas, and that the fossils derive from the middle and late Paleocene, Eocene and Oligocene. It is clear, however, that these primates are certainly deserving of more careful attention than they have been given. The systematics of the New World primates have not yet been correlated, for example, and the poor taxonomic principles which are currently used are actually an obstruction to understanding the biology of primates (see Buettner-Janusch 1966:257). Eventually we will need to approach such questions as the total environmental setting of early primate development, the relationships between the early primates, and especially the relationship between the early primates and the living primates of today. What forms of the past are ancestral to those of the present? We know very little of these relationships at present.

It should be obvious, even after a brief examination, that relative to the respective inheritances of Old and New World scientists an immense discrepancy exists. Compare several million years of human evolution, and all its branches

and ramifications and physical data, with a few thousand years of development at the most recent end of the evolutionary scale. Clearly, American physical anthropologists have found themselves at something of a disadvantage, relatively speaking, with regard to the availability of specimens and subjects for study. Viewed on the individual level, through the eye of a single scholar, the problem is minimized. For there is such an immense amount of work to be done, given our present resources and research topics, as to defy hopes for completion. In the overview, however, we must go beyond the research hours of an individual scientist, to the goals and purposes and accomplishments of the discipline as a whole. Hence it is advisable to assess the general direction and state of affairs of the monolithic phenomenon which is an academic discipline. Such an assessment prompts us to ask, finally, "What do American physical anthropologists have to call their own, besides the American Indian?"

The answer lies in the modern approach to human biology. It relates to what physical anthropologists do today and how they spend their time and energy. It is integrally a part of the "new" physical anthropology and its insistence on problem-orientation, as examined in the last chapter and elsewhere in this dissertation. What American physical anthropologists have to call their own, in addition to the wealth of American

Indian data, is explicitly the way they look at things. Perhaps the most significant and potentially valuable possession of American physical anthropologists today is their orientation toward understanding function and process, rather than simply describing morphological structure. The direction of research today permits a great percentage of studies to be conducted without the traditionally necessary collection of skeletons. Today we are looking not so much toward amassing mountains of raw data, as has been done in the past--an enterprise which consistently and continually demands ever more skeletal populations--as toward re-examining old projects and old assumptions, in the ever-revealing world of in-depth thinking. That is, the modern American physical anthropologist seeks not so much to find new populations to compare, as the Eskimos and mound-builders and Tierra del Fuegans have been compared, or even to find new ways to compare the same populations, but rather to go back and start over, asking how, when and why the differences came to be.

The modern approach to physical anthropology makes especial use of the heterogeneity built into the American population at large. The United States--and, in fact, the whole New World--has traditionally been characterized as a "melting pot." Individuals and groups from all parts of the world have relocated in America during the past four centuries,

with ensuing race mixture which might never have occurred otherwise. Physical anthropologists and human biologists have long recognized that this unique population presents bountiful research potential. Many early studies--particularly those of Boas, Hrdlicka and Hooton--focused on particular ethnic populations in the New World, as well as mixed and altered populations. Both of these aspects of a racially heterogeneous population have been pursued to the present day.

Emphasis on genetical approaches to problems in human biology and race since 1950 has furthered our knowledge of racial differences, particularly with reference to the heterogeneous American population. The infusion of genetics into physical anthropology has brought about the resurrection of some old problems and the presentation of new, previously-unconceived questions. And as the genetical field of inquiry has developed, and genetical techniques have been perfected, increasingly more questions have been answered regarding man and the processes of his existence. The cumulative result is that we are understanding more of the overall picture of evolution and life. Drawing from genetics and in turn contributing to that field, American physical anthropologists have accomplished some of the more important studies in human genetics during recent years.

The unique New World population has fostered studies in environmental adaptation which are particularly American in scope. The adaptation of Eskimos to their demanding environment, the adjustment of Peruvians to high altitudes, and the physical ability of Tierra del Fuegans to meet, with little protection, the rigors of their climate, have provided workshops for the physical anthropologist. From such investigations, which are reviewed at greater length in the next chapter, has come a deeper understanding of man's interaction with his environment, with consequent illumination of aspects in man's long prehistory.

The specifically American premium placed on the identification of unknown dead--as discussed earlier and in greater detail in the next chapter--has provided for yet another anthropology workshop. Until recently Europeans have not given special attention to individual identification, as have Americans since the Revolutionary War. As a result of the demand for individual identification--whether the victim derives from war, homicide or accidental death--a variety of research techniques and procedures have been established. A by-product of this enterprise has been knowledge and data which are directly applicable to other aspects of human biology--integrating, in effect, human identification with the greater physical anthropological endeavor.

The question to be answered here, for satisfaction of the fourth prerequisite, is whether a specific category of physical specimens exists for the use of American students of physical anthropology, and whether they have utilized these resources to maximum potential, overcoming the inherent limitations. We have already established that there are "American physical anthropologists." The question now is whether they function within a framework of a distinctively American discipline or within an Old World structure. The manner in which our scientists have approached the availability of resources and have directed their research orientations indicate that their actions and procedures are indeed justified. That is, they have maximized and exploited the data and specimens available, first salvaging the more perishable elements, and have delimited a range of studies which are uniquely appropriate to the geographical, political, social and cultural environment in which the studies emerged and are pursued. First securing a maximum of information from living populations, and storing, in the process, vast amounts of raw data, American scholars built collections of skeletal remains, which are extant for study and investigation. Then they returned to studies of living humans. Finally, they have delved into function and process, and have freed themselves from the confines of osteological collections. Growth and constitution

studies, for example, which are so prevalent in anthropology today, utilize as subjects any and all living beings. Consequently, as anthropologists continue finding and developing their own workshops, the necessity for "possessing" a body of specimens, for the purpose of insuring quantity and quality in a discipline, is not so great as before. Orientation to materials assumes importance when a body of physical specimens is deficient.

The overall trend has had a liberating effect on American physical anthropologists. From this they have benefitted and have provided returns in terms of more useful studies. In so far as the very character of American physical anthropology, as distinct from European branches, reflects the availability and use of specimens and potential, therefore, the American branch may be regarded as distinct and valid by definition. Demonstration of and justification for a separate existence, that is, are found in history. Consequently, justification for the continuation of a distinct academic enterprise is found in the history and success of dealing with the problems at hand.

CHAPTER VI

THE ORIGINAL RESEARCH

Although there are many ways to view the history of physical anthropology, perhaps the most obvious is the tangible result of all past labors. This is the body of original research. It was suggested in the fifth prerequisite that any discipline will have accumulated a body of raw data, original research, and the relevant studies, which will constitute the peculiar orientation of that discipline. This prerequisite should be easily satisfied, for we have only to go to the vast literature in order to demonstrate that there is a body of research which is peculiar to American physical anthropologists. The discussion which follows is organized according to the major sub-areas of the discipline.

Race

If the study of the phenomenon of race was critical in the development of the United States, it was no less critical in the development of American physical anthropology. The foregoing chapters have amply demonstrated that the nature of human races was preoccupation for a great many early physical anthropologists. Interest in placing the aboriginal

populations of the Americas categorically with relation to European and other races was of foremost importance. Such a bent was natural for the times. That is, considering their understanding of the reasons, meaning and implications of racial differences in mankind, in conjunction with the novelty of newly-encountered populations, it is understandable that scientists directed their efforts to understanding race. In addition to their curiosity about American aborigines, they were intrigued by the native Africans who were brought by slave trade to North America. The origins of these people--so little understood, yet so widely conjectured--as well as their probable biological and social influence upon Anglo-Saxons in America, were matters of grave importance academically, scientifically, socially, politically and philosophically. In retrospect, then, everyone, regardless of his orientation, had a vested interest in the resolution of these vexing questions. Involvement by association was generally a matter of fact.

The task of resolving racial ambiguities was assumed by physical anthropologists, through implicit common consent, as an appropriate long-range goal. The explanatory and definitive comments by Boas (1940) illustrate the depth and scope of anthropological involvement. Boas clarified questions appropriate to the scientific study of race, as well as the extent

and scope of anthropological involvement therein, in effect providing a succinct resume of early thought.

The general progress of this investigation, as shown in the researches of a broad spectrum of scientists, may be found in the early issues of the American Journal of Physical Anthropology. Through 1929, spanning the first twelve years of the Journal's publication 109 notes pertaining to race appeared in the "Literature" of the Journal. In addition, 47 full-length articles appeared relating directly to racial studies. Eleven of these articles concern racial crossing or mixture. The remainder deal, for the most part, with specific racial characteristics and differences. In this respect the scientific, analytical trend of physical anthropologists is demonstrated. Whereas earlier "ethnologists" had devoted countless volumes to discussing and theorizing about raciology, twentieth century physical anthropologists undertook to study individual questions in detail. Specific racial characters were isolated and examined with regard to variability in occurrence and manifestation, causation, distribution, and so forth. Scientific method was, in fact, being introduced into physical anthropological research.

Although skeletal characters were investigated with some regularity during this period, only two articles related to post-cranial bones, namely, age changes in the pubic bone and

iliac differences. The comparative investigation of cranial characters comprises the remainder of osteological studies on race. Among the topics are cranial height, breadth and length. A 1919 anthropometric study by Hrdlicka investigated the possibility and validity of recognizing racial types (1919a:413). Somatometry studies were concerned with both comparative mensuration and surface description. Facial height and width were investigated as criteria of race, as were blood, heart, kidneys, intestines, liver, spleen and muscles. Hair was studied in terms of pigmentation, weight and body distribution.

Most notable, from this brief survey, is an orientation toward physical description and mensuration. The trend, as we have seen, was typical of the times. Physical anthropology was but in its infancy, as goes the relative development of academic disciplines. The purpose of these efforts was to record fact--raw data for the accumulation of a useable body of information. Cause and effect and functional processes were not regularly investigated during the first three decades of the twentieth century. Deeper questions of functional significance were to await a more intrinsic structuring of the science. This structure was to consist of a large accumulation of data, diversification of interests, satisfaction of preliminary interests, and eventually a concentration on

questions of primary interest. More specifically, however, the introduction of genetical studies brought about a broadened scope and maturity in the discipline.

In time the orientation toward race itself changed. Research during the earlier years provided extensive and particular knowledge on racial differences. This knowledge became generalized through theory which was read from the gamut of isolated and specialized studies. Eventually the scientific tenor evolved from an investigation of curiosities for the satisfaction of general interest, to the formulation of research for solving laboratory and social problems. The osteologist precipitated this development. The need for measurable criteria, with statistical accuracy, was observed in the identification of human skeletal material which reached the laboratory. Perhaps the deeper reason may be attributed to the idea that physical anthropology was finally coming to be of service to the wider academic world and to mankind at large. Law enforcement agencies found that they could call upon physical anthropologists to identify unknown human skeletal remains. Consequently, crime detection was furthered. The body of research which emerged to meet this need has been summarized by W. M. Krogman in a laboratory handbook, The Human Skeleton in Forensic Medicine (1960). The research which Krogman includes spans an 80-year period,

demonstrating, in effect, the gradual solution of identification problems and the refinement of techniques.

Relating one race to another, and in turn focusing on the distinctiveness of each race, some scientists emerged with the concept of "pure races." Believing that races were independent in origin, biologically specific, they directed their efforts toward demonstrating the validity of the pure race concept. The movement, however, failed in its efforts, for the theoretical fallacy of the concept was demonstrated in time. We know now that crossbreeding throughout all time periods has made pure race impossible. The UNESCO Statements on Race (UNESCO 1951, 1952) were particularly instrumental in demolishing unsound theories of race, particularly those lines of thought which had, through the years, given vent to racial prejudice and discrimination. Dispelling the preconception of pure races did not sound the downfall of race studies, on the other hand. It rather complicated the study, in terms of introducing the necessity for defining racial differences. The growth of osteological study, described above, was but one facet of the endeavor to understand the meaning of race.

Another facet was the establishment of a Standing Committee on Race Relations at the Thirteenth Annual Meeting of the American Association of Physical Anthropologists in 1942. W. W. Greulich introduced a motion calling attention to the

generally poor treatment of racial minorities. His motion was accepted--a fact which shows, perhaps, physical anthropologists' awareness that they have, indeed, a capacity for contributing directly to knowledge and understanding in the world beyond academia. The pursuit of pure research tends to remove scientists a step from the actualities of everyday problems. A movement toward establishing contact with the outside world and assisting in world problems is, on the other hand, a tacit acceptance of relevance to everyday problems and a willingness to contribute to the direct solution of problems.

A further step in this direction was the involvement of anthropologists in drafting the UNESCO Statements on Race, the importance of which has already been suggested. The first statement, an extensive 15-point analysis of scientific opinion, was prepared in Paris in 1949 and was widely published (see UNESCO 1951:142-145). A panel of eight scholars drafted the statement, which was subsequently revised by Ashley Montagu, a member of the panel, when professional criticisms poured in. A great deal of controversy arose as a result of the statement. Acceptance in varying degrees, as well as several points of contention, are discernible from a series of Correspondences published in the 1951 issues of the British journal Man (see Vol. 51, pages 15-18, 54-56, 87-88,

103-104, 115, 131). T. D. Stewart (1951a) attributed the confusion, which was worldwide in scope, to a scientist-layman discrepancy in interpretation. That is, the public was requested by the Statement to discount visible racial differences and to accept a genetic basis for racial determination. "The public has a right to be resentful," Stewart said, "when it is told that race is a myth or a fallacy."

Assuredly, public opinion was of prime importance in this matter, since the object of the UNESCO Statement was to further public understanding of race and to combat racism and racial discrimination. Clearly, the Statement was unacceptable to scientists. It was repugnant to many interested laymen. Consequently, a second UNESCO Statement on Race was drawn up. A number of specialists, comprised of physical anthropologists and geneticists, deliberated to produce a more acceptable statement. Resultant was the 1951 Statement on Race (see UNESCO 1952). The Statement again was extensive. It was also more explanatory in nature and more generally acceptable.

That the layman could not accept a world without race is hardly surprising. For most scientists have also come to see that there is validity in the concept. There are biophysical differences in the world's human populations, and it is essentially to the study of these differences that the term

race applies. Consequently, in order to devise a more workable and realistic concept, anthropologists and human biologists have turned to questions of taxonomy. Classification of mankind into groups has been an activity of man throughout history. With the increasing sophistication of morphological and genetic studies, however, racial classification assumed new significance. Juan Comas, in the Manual of Physical Anthropology (1960), has provided an extensive and valuable summarization of classification systems which have emerged since the late nineteenth century. His survey demonstrates the great variability and relativity of race, of the arbitrary classification of man--a situation which continues to the present day. He emphasizes also the meaning and definition of race which began to emerge during this recent period--that racial classification is based upon generally-accepted characteristics and the arbitrary grouping of these characters, and that a given classification never includes individuals of identical somatic or genetic constitution. Rather, race--as we understand it now--is a mass or complex of variable traits arranged along a continuum.

Despite these modern and continuing contributions to anthropology, the assertion has been made that racial studies are no longer a valid enterprise for physical anthropologists. Buettner-Janusch, suggesting that physical anthropology must

remain a bastion for pure scientific procedure, and that the study of racial differences may not be conducted according to satisfactory scientific procedure, concludes that race must be forgotten (Buettner-Janusch 1969). This line of thinking is curious in light of the history of race studies which is presented here. The meaning and importance of racial differences has traditionally been of significance to anthropologists. It has been a special domain of anthropology. And while we cannot use the past alone as a basis for justifying the study of race now, we certainly cannot throw out race so easily as Buettner-Janusch would like. Likewise, we cannot dispose of a problem so easily, with the excuse that current techniques are inadequate to solve the problem. Furthermore, the genetical approach to questions which pertain to race--an approach which Buettner-Janusch champions--is in fact beginning to provide new avenues for answering old questions. Since the midpoint of this century the study of race has assumed new and previously unparalleled importance, as interaction with biologists and geneticists has led physical anthropologists into new research areas.

Not only is the future of racial study insured, therefore, through new research potential, but the possibility for an anthropological contribution to solving some plagueing problems of the modern world is also enhanced. The importance

of pursuing the elusive nature of race, in other words, has increased rather than faded. In this capacity physical anthropologists may serve mankind. The stakes are far too great for us to turn back now.

Anthropometry

The historical survey of American physical anthropology, presented in Chapter III, revealed that the origins of the discipline lay in a European heritage and a response to American needs, and that this field of inquiry was at first called "ethnology." The science of ethnology rested essentially upon techniques of gross visual observation, measurement and comparative mensuration of both the human body and the human skeleton. The techniques of measurement and the processing of the resultant data have come to be called "anthropometry," a term which includes "osteometry." Anthropometry is a technique, therefore, which is at least as old as anthropology itself. As a systematized method for measuring and observing all parts, extremities and organs of the human body, accomplished with the aid of a large series of accurate and highly refined instruments, anthropometry is limited only by the nature of the problems to which it is applied, and by the subjects utilized. The possibilities in anthropometry are

essentially endless, bounded only by the researcher's ingenuity and resourcefulness. Yet, as Juan Comas has so ably admonished (1960:254 ff.), utmost caution must be used if anthropometry is to remain the technique it was designed to be. Anthropometry is not the end, but rather the means to an end--one of the most effective means for accomplishing human description and comparison.

The worth of anthropometric endeavors to the progress and future of physical anthropology has not gone without challenge however. John Buettner-Janusch, in a scathing challenge to transcend old methods, and move exclusively to the realms of genetics, says this about anthropometry:

> It is no longer satisfactory for a student to measure a pile of bones and calculate means and variances and come up with what I consider a spurious conclusion about the relationship between one pile of bones found under the dirt in Indiana and another pile of bones found under the dirt in Ohio. You will pardon the sarcasm in inverse proportion to the degree to which you are interested in old bones. I am deeply depressed by the fact that, although over a hundred years of detailed investigations of moldy old bones (skeletons) have been monumentalized in large monographs usually residing in the basements of university libraries, the addition to knowledge in general and to the advancement of physical anthropology in particular is almost nil. For all the good this work has done, the bones might just as well have been ground into meal for the rose bushes on university grounds (1969:134).

Buettner-Janusch places his concepts of the current needs of physical anthropology within the model of academic evolution.

Envisioning an adaptation on the part of scientists, from "the so-called organismal or traditional and the molecular approaches" to a more complex and integrated level than previously experienced, he speaks of natural selection in academia which "is truly red in tooth and claw," moving from the past in a new adaptive radiation to a new adaptive plateau.

Buettner-Janusch's statements seem, however, to be more an overstatement of opinion than a sober analysis of reality. He is surely correct in assuming that anthropologists must move to more scientific realms, and utilize the newest of biological and genetic method and procedure. For physical anthropology is indeed expanding. His one-directionalism, however, seems to be his error. For who could even suppose that osteological measurement is no longer appropriate to the physical anthropologist's use of his time? Who could suppose that gross physical measurement no longer is a valid research tool? And as far as his implication that osteometric analyses have been to no avail for all these years, it can at least be said that many other anthropologists interpret the past rather differently.

We shall accept, then, that anthropometry is indeed an integral part of physical anthropology, and move on to the history of these studies. The real origins of anthropometry lie far back in history, at some point when observable

differences in human populations of the world stimulated a curious observer to measure and compare. These origins are lost in time. There were other factors in the emergence of the anthropometric technique, however. The worldwide expansion of European states during the past four centuries has increasingly shown the scientific world an extreme variation in human types. Seeing these differences has prompted man and science to investigate the extent of variation. The appearance of fossil forms, mainly in the Old World--life forms which had to be explained in one fashion or another--likewise impressed upon minds which were ready to see, that enormous changes had occurred in man's physical history. These variations in fossil forms were also approached through anthropometric techniques.

In nineteenth-century Europe these investigations were systematized into the science of paleoanthropology. Physicians, anatomists and natural historians, with the assistance of a great many personnel from other walks of life, both educated and uneducated, guided the development of the field. American scientists with similar interests, and with similar problems, followed the pioneering efforts of Europeans, in their attempts to deal with prehistoric human remains in the New World. And here, as in Europe, physicians, anatomists and natural historians were principally responsible for guiding the

development of "ethnology" and its most essential techniques, osteometry and anthropometry.

In the early 1800's, however, anthropometric techniques consisted solely of isolated and un-unified measurements, with hardly a notion of the systematic mensuration of the entire human body. The work of John C. Warren, a physician who, investigating North American Indian skeletal remains, laid the groundwork for the American development of physical anthropology (see Chapter III), shows clearly that some few measurements were taken as they were conceived. The noticeable feature is that so little research had been accomplished in this field, with the result that only gradually did scientists begin to record identical measurements, a standardization which first requires the definition of osteometric or anthropometric points and loci. By the time Samuel G. Morton began probing the racial differentiation of human populations, primarily with respect to American Indian skeletal remains, a handful of standard techniques had been accumulated. With these guidelines, in conjunction with those which he originated, Morton was able to compile a valuable volume of osteometric data. His works, well-grounded scientifically and widely circulated, encourated other physicians and interested persons to engage in "ethnological" research.

After about 1850 or 1860 osteometric studies increased rapidly in quantity--and, it may be said, in quality also. For as research reports were published, and were passed from hand to hand, a common core of measurements began to emerge. The general survey of historical developments given in Chapter III actually embodies the emergence of osteometric and anthropometric technique. This historical survey shows also that while first attempts in measurement of man were concentrated on skeletal remains--almost wholly on the American Indian, in America--the fuller development of anthropometry, including the measurement of the living body, awaited the arrival of Boas. As a result of his initiative and stimulus, this field also grew.

Evidence that standardization was gradually becoming a reality in the twentieth century is found in Harris H. Wilder's publication, in 1920, of A Laboratory Manual of Anthropometry. Wilder's Manual apparently was the first such work, of major proportions and importance, to emerge from American resources. It is both historical and descriptive in nature, concerned with anatomical points, measurements and indices, and how these factors have been derived and perfected through time. In these respects, his work remains as one of the best early references on the measurement of man.

As in so many other areas of physical anthropology, Hrdlicka ranks among the foremost anthropometrists of all times. His earliest orientation in physical anthropology, the factor which in large part enabled him to successfully sever physical anthropology from other academic disciplines (medicine, anatomy, biology, etc.), was the technique of anthropometry. Physical anthropology was a measuring science in the early decades of the twentieth century, and most of the research conducted under the auspices of the discipline was based on comparative mensuration. Through concerted effort and years of practical experience, Hrdlicka was able to assume a stature of the greatest magnitude in the measurement of man. Most of his hundreds of publications, some of them quite lengthy, either concentrate upon or draw from the anthropometric technique. A landmark in the field appeared in 1939 with his Practical Anthropometry, published by the Wistar Institute. This volume survives to the present, in its revised form (Stewart 1947), as one of the primary manuals of anthropometry--a factor due obviously to its completeness, accuracy, and general acceptability. A number of other manuals have joined Practical Anthropometry in presenting a unified and well-integrated, well-standardized view of the current status of anthropometry. As a result of the advanced state of anthropometry, most modern manuals and summaries

present new orientations and uses of data which has essentially become standardized.

The variety of techniques related to and drawn upon by anthropometry are widely used today. Anthropometry is commonly taught as a basic course in advanced physical anthropology. Fieldwork is regularly conducted at many localities across the country. The modern approach to osteological study is well demonstrated by the researches of William M. Bass and his students. Excavating on a large scale in South Dakota, Bass has assembled a collection of some 1800 skeletons, making this collection one of the larger available for comparative purposes. Monographs growing out of this collection illustrate the current trend in maximizing information from the skeletal record (see Bass 1963, 1964a, 1964b; Krause 1969; Bass, Evans and Jantz 1970; Jantz 1970; Lyon 1969; Steele 1970). The continuing search for relationships of skeletal characters between and within populations, pursued through the modern aids of statistics and computers, will build the record of accumulated data, and will feed information to those anthropologists who are researching mechanisms through which attributes are genetically linked.

Paleoanthropology

According to the definition of the scope of the discipline, as presented in the first chapter, the study of fossil man is specifically appropriate to American physical anthropology only when it relates directly to American or New World fossils. American students of the discipline who conduct paleoanthropological researches in the Old World, in other words, operate in a distinctively Old World framework which is peripheral to the central concerns of American physical anthropology. No truly ancient fossil remains have been found in the New World and the character of all American fossils places them unquestionably within the contemporary species. Hence, as both Vallois and Comas have noted, "the study of the so-called American fossil man more properly belongs in the field of physical anthropology rather than to paleontology" (Comas 1960:542-543).

A number of cultural sequences have been defined in early periods of American archaeology, and the existence of prehistoric man is confirmed by a few fossils. Due to the pronounced lack of productive and carefully excavated sites where fossils are found, however, cultural sequences have not been well correlated with prehistoric man finds. Sellards described in 1916 two finds of human remains from Florida.

Stewart (1946) has found the skulls are dolicocephalic, and Heizer and Cook (1952) suggest that the individual has a Pleistocene date. Minnesota Man, a 15-year-old female, has remained an enigma since its 1931 discovery by Jenks (1936). The stratigraphy, which is disputed, may be Pliocene or Pleistocene, although the former date is beyond any credibility. Browns Valley Man was also found in Minnesota, in 1933, and like the earlier find its stratigraphy was also uncertain (Jenks 1937). Other human remains have been found in Texas, New Mexico, Wyoming, Minnesota, Mexico and elsewhere. In each case there is little enough certainty regarding dating and stratigraphy to render the finds anomalous and puzzling.

The remains of Tepexpan Man, discovered near Mexico City in 1947, were found in direct association with fossil mammoths. The stratigraphy is identified with the Mankato-Cochrane stadial of the Wisconsin glaciation. Although Romero has described the individual as 55-65 years, with Homo sapiens characters which resemble those of later periods in the same area, and Stewart and Weidenreich have confirmed the analysis (deTerra, Romero, Stewart and others 1949), Genoves and Romano (1958) have more recently suggested that the individual was only 30 years of age, and smaller than originally calculated. The issue has not been resolved.

The above example illustrates that while there are a number of fossil forms which are suggestive of early occupation of the New World, precise and unequivocal dates are difficult to ascertain prior to about 10,000 B.C. Physical anthropologists and archaeologists are attempting to solve these problems together. The limit of the physical anthropologist's statements, at the present, relate to the early presence of Homo sapiens, with some variations, but generally similar to modern-day populations.

Almost as soon as the American Indian came to be understood as a race of people different than Europeans or other Eastern Hemisphere populations, questions regarding his origin were circulated. From this early interest developed scientific interest in tracing the development of New World populations. The natural course of development of this interest has resulted in a continuing search to validate the early presence of man in the New World. And while today we are certain that the Bering Strait theory of the peopling of this hemisphere accounts for the bulk of population migration, both human and mammalian, from Siberia (see Bryan, 1969, for a thorough and well-documented survey of the evidence), yet there are numerous problems attending our understanding of the history of Old and New World connections. While we have intricately studied the geology, geography, climatology and cultures of Siberia,

Alaska, and surrounding areas, yet there are other questions which must be answered. Sharon S. McKern and Thomas W. McKern (1970) ponder in a recent article, "Odyssey: The peopling of the New World," numerous similarities and coincidences in cultural and societal expression, many of which can hardly be explained in any adequate fashion by a rather simple crossing of the Bering Strait. So many considerations must be weighed, not the least of which is the temporal factor, that we are nowhere near understanding the full implications of the peopling of the New World.

The net impact of scientific thought regarding the antiquity of man on this continent was for many years in the hands of Hrdlicka. Firm in his belief that man was a relative newcomer, he went to extraordinary lengths to demonstrate his belief. Two monographs mentioned earlier (1912, 1918) evidence the scientific ability with which Hrdlicka buttressed his views. Writing popularly, for newspapers, and lecturing, he also influenced lay thought on the matter. His opinion was, essentially, that there was insufficient evidence--morphologically, biologically, geologically or otherwise--to warrant an initial migration before about 8000 B.C. The temporal question has been one of the most recurrent factors, and today it is generally accepted that at least 40,000 years must be allowed to account for geographical dispersion,

physiological differentiation, and cultural divergence of the American Indian.

Identification of Human Skeletal Remains

The particular knowledge and techniques which physical anthropologists possess have enabled them to engage in some practical and applied problems. The application most structurally integrated into the discipline is that of processing and identifying human skeletal remains. In building up their skills and in increasing the data record, in other words, anthropologists have answered questions which not only they ask, but which other persons ask them.

The physical anthropologist's special contribution to identification is with skeletal remains. Few anthropologists have developed competency to deal with soft tissue remains, leaving these investigations to anatomists and physicians. The extreme rarity of his encounter with other than teeth and bones has precluded this extension of the physical anthropologist's energies. With regard to teeth and bones, however, he is the specialist.

Identification services are rendered regularly to law enforcement agencies, by some anthropologists. The medico-legal aspects of a skeleton unearthed or discovered on the

countryside may be complicated and ramifying. A skeleton may find its way to a local sherrif, local police, state or Federal Bureau of Investigation. In any case a nearby physical anthropologist is often consulted whenever standard legal procedures do not provide identity of the individual. It has become regular procedure at the Universities of Kansas and Texas, for example, for remains to be received for professional identification. There are other legal problems which the physical anthropologist may approach indirectly. Payment of life insurance benefits and probate, for example, depend on the availability of a death certificat which, in turn, necessitates identification.

Faced with such a situation, the anthropologist seeks to give as many identification clues as the evidence will allow. He remembers, however, that his information to the legalist is of a critical nature; it is necessary that this information be defensible in court. Difficulties arise at this point. The legalist needs to know precise information based on infallible evidence. His need is to say, this is or is not John Doe. The anthropologist cannot ultimately make such statements. His expertise allows him to say, for example, that a given skeleton "has indications of being male, 30-35 years, 5'7" stature, Negroid, with certain abnormalities and a possible disease record." In this context, Krogman raises

the question, "How valid, to begin with, are our so-called norms for age, for sex, for stature...?" (Krogman 1962:4). For our statistics are based, in large part, upon biased samples. There simply are no skeletal collections which broadly represent the entire population. Such a collection is, by definition, virtually unattainable, for the population at any given point in time and space is extremely heterogeneous. The strategy, therefore, has been to work around these and other inherent problems, to the attainment of needed data and results.

Not all identification cases presented to the anthropologist relate to individual deaths. Mass deaths require special systems of processing and analysis. Although such cases are rare, precedents for procedure have been established. In consequence of the Korean War, and the repatriation of war dead from North Korea by the United States, Thomas W. McKern and T. Dale Stewart (1957) engaged the task of cleaning, sorting, identifying and reporting 450 male skeletons.

The particular study illustrates well the scientific importance of having large, fully documented population samples. Compared to anatomical collections, which are biased toward older age and lower economic brackets, the war dead sample from Korea embraces a male population of approximately 17 to 50 years, with a more heterogeneous socio-economic

distribution. Clearly, both types of collections encumber difficulties. Yet they serve dual ends of a single purpose: the attainment of more accurate, complete and reliable indicators of vital statistics.

The McKern-Stewart study resulted in the accumulation and interpretation of skeletal age change indicators hitherto unknown, and permitted, for the first time, checking previously-utilized techniques of identification. Virtually complete medical records--probably less susceptible to error, accidental or non-accidental, than medical collections--were available for each individual. This corroborative evidence allowed not only for new statements regarding progressive maturational phenomena, but also for corresponding statements regarding previously suspected or hypothesized phenomena. The monograph serves both scientific and practical needs. The practical aspects are accentuated by the presentation of charts for evaluation of skeletal maturation--by those anthropologists using the work as a guideline in their own research--and through the introduction of a set of pubic symphysis models for age estimation. The latter is a device which has gained rather widespread acceptance since its innovation.

The sexing of human skeletal remains is traditionally a problem of considerable magnitude. There are no real

standards. Personal experience and ability remain the controlling factors even today. Krogman, in assessing the subjectivity of sex determination, suggests "that all estimates should be lowered 5-10%, depending on the relative completeness of the material to be sexed" (Krogman 1962:112). That is, he feels his accuracy is about 90 to 95 per cent of all cases. Stewart suggested a similar proportion of accuracy, though that proportion was diminished to 80 per cent when the adult skull alone was available. Similar figures apply to Hrdlicka's attempts.

The means whereby these estimates are reached involve both gross morphological observation, including personal evaluation, and statistical and other mathematical formulae which have been developed for this purpose. As early as 1914, for example, Pearson was investigating these problems (Pearson 1914-15). Most of the research relied upon today, however, has proceeded out of the past 25 years. Boucher (1955, 1957) has investigated the problem with relation to foetal remains. Hunt and Gleiser (1955), working with bones and teeth, have provided guides for preadolescent remains. Reynolds (1945, 1947) studied the pelvic girdle of infants, as well as prepuberal children. Washburn (1948, 1949) investigated sexing problems from the standpoint of the pubic bone. Thieme and Schull (1957) approached the entire problem of sexing,

concerning the whole skeleton. These studies indicate, however, that the greatest accuracy in sexing is to be found through use of the pelvis or, in the absence of a complete pelvis, the pubic bone.

The ageing of human skeletal remains is a much more complex problem--so complex, in fact, that Krogman devoted to it three chapters of The Human Skeleton in Forensic Medicine. Age estimation is based almost entirely upon skeletal maturation guidelines and epiphyseal closure. Todd, pushing for firm criteria over 40 years ago, investigated growth and development, and general maturational activity in the skeleton (Todd 1931, 1933). Greulich and Pyle (Greulich 1951, 1958; Greulich and Pyle 1959) have done pioneering work in this area, especially with regard to radiographic analysis of maturation in the hand and wrist. Pyle and Sontag (1943) and Pyle and Hoerr (1955) have done similar work, and McKern and Stewart (1957) have pioneered in a different vein. McKern (1957) has approached the question with the concept of combined maturational activity in the skeleton.

Outside of the general attempts, ageing is approached from two skeletal foci. The first, the skull, now appears to be less reliable. Dwight, in 1890, attempted to relate suture closure to age. Parsons and Box (1905) made another early

attempt at ageing by suture closure. Montagu followed with a study in 1938. And as late as 1953, Singer produced a study on age estimation from cranial suture closure. Most emphasis now, however, is on the pelvic basin. Todd produced eight studies on this subject, which are subsumed in six publications (1920, 1921a, 1921b, 1921c, 1923, 1930). He defined the pubic symphysis and plotted its age changes, in terms of descriptive osteological morphology. Brooks, in 1955, reinvestigated Todd's criteria and provided minor modification. Major revamping of pubic symphysis evaluation came about in 1957, with McKern and Stewart's Skeletal Age Changes in Young American Males.

As a part of human skeletal identification procedures, stature is calculated on the basis of long bones. Although Beddoe attempted stature calculation on English remains in 1887-88, the first such study of serious import was Manouvrier's study in 1893. Pearson followed with the well-known statistical study in 1899. Their work, although significant at that time, has long since been superceeded. Dupertuis and Hadden (1951) and Telkka (1950) predicted human stature. The most usable and most widely accepted guidelines, however, are found in Trotter and Gleser's work (1951a, 1951b, 1952, 1958). Wells reassessed the notion of stature estimation in 1959.

The denotation of racial attributes in the skeleton is a much more subjective and hazardous venture than are estimations of sex, age and stature. Racial affinities, in the first place, relate to a clustering of attributes rather than hard and fast criteria. These clusterings may or may not correspond with an individual's trait accumulation. Hence, the difficulty. In most cases, if statements regarding race are made at all, they find their basis in a few pronounced characteristics--characters which, with time and the scientist's experience, have attained a certain degree of respectability and reliability. Yet, as Krogman admonishes, "In the discussion of race differences in the skeleton it is impossible to do more than sketch in the broad outline" (1962:189).

Krogman (1962:206-207) provides six summarizing statements regarding the study of race differences. (1) American white and American Negro groups are best differentiated, as based on studies of the Todd and Terry collections. (2) Based on the skull, accuracy should be attained in 80 to 90 per cent of cases; discriminant analysis should yield over 90 per cent accuracy. (3) The mandible, exclusive of dentition, cannot be racially classified. (4) The pelves of whites and Negroes should be differentiated in 70 to 75 per cent of cases. (5) Long bones are too susceptible to variation and cannot be relied upon. (6) The scapula (despite several attempts) cannot be racially classified.

Krogman's statements summarize, in effect, what has been done, what has been accomplished. And the accomplishments are impressive, considering human variation, the breadth of factors relating to human skeletal identification, and the odds against which these scientists work. Physical anthropologists may well be pleased with their century's work in this field. Yet many questions remain to be answered, and techniques to be perfected. Human skeletal identification provides an avenue of practical application, through which society may benefit from the physical anthropologist's labors. If for no other reason, research should continue in this field.

Paleopathology

Since the purpose of physical anthropology has been to study man, a bio-social organism, from all points of view, it was a logical step for physical anthropologists to delve into prehistoric man's diseases and injuries. The outgrowth of this curiosity was the building of a new science, paleopathology. In this study, anthropologists or human biologists are drawn to consider the implications of culture and ecology for man's physical existence. Paleopathology is, then, a field of study which unites diversified interests, to the attainment of mutually useful answers (see Jarcho 1966; Armelagos 1967; Kerley and Bass 1967).

Rudolf Virchow was among the first scientists to study disease, with orientation toward a systematic understanding of processes and manifestations. He understood disease in general as nothing else but "life under changed circumstances" (1862:33). The integration of paleopathology with physical anthropology followed in 1896 when Virchow diagnosed the Neander Valley fossil as abnormal. There is a distinctively American character, however, to subsequent developments in paleopathology.

Saul Jarcho has summarized, in considerable detail, the development of inquiry. He traces the origin of paleopathology back to 1822, when John C. Warren, a medical doctor in Boston, published A Comparative View of the Sensorial and Nervous System in Man and Animals. He described here examples of artificial cranial deformation. In 1839, Samuel G. Morton's pioneering abilities produced the well-known Crania Americana. In this volume Morton pictured artificial deformation and obvious trauma.

Ralph Moodie's publication of Paleopathology: An Introduction to the Study of the Ancient Evidences of Disease, in 1923, is a landmark in paleopathology literature. The first major summary, Paleopathology served long as a systematic, authoritative and definitive reference in paleopathology. No comparable synthesis has appeared since this time although,

as we shall see, research and syntheses of lesser scope are currently appearing prolificly.

Jarcho (1966:23) has suggested that the first three decades of this century constituted a golden era in paleopathology. In addition to Moodie, others contributed to the literature--Ruffer, Williams, Hrdlicka, Hooton, and others. Certainly it was a progressive era. Yet it is in the three decades immediately past that the science has achieved its present scope and direction. Goldstein (1963:100) notes, for example, that Hooton's The Indians of Pecos Pueblo (1930) led the way to studies in pathologic frequency in populations and of suggesting certain life conditions which possibly accrued from pathologic manifestation. During recent years numerous questions have been answered, regarding prehistoric man's ailments, and numerous techniques have been devised, toward the solution of still more problems.

Certain diseases and questions are recurrent. Among these is tuberculosis, a disease which may be traced back to about 5000 B C. in the Old World. Progress in understanding the disease, in its various manifestations and implications, is shown in the sequence of research and literature published (see, for example, Klebs 1917; Elliot Smith and Dawson 1924; Ackerknecht 1962; Goldstein 1963; Putschar 1966; Jaffe 1966). The origin and antiquity of syphilis are other questions to

which paleopathologists have addressed themselves. The disease and its origin now have become legend, and even now we are not likely nearing a clearcut answer. For the ramifications are too extensive, and the complexities too ingrained. There are problems in simply defining or in diagnosing the disease, especially in prehistoric remains. The literature is replete with arguments on diagnosis of syphilis.

A great deal of mental effort is now being directed toward the idea of establishing a registry for the examination and recording of prehistoric remains. The pervading problem lies in that communications among paleopathology researchers are inefficient, often non-existent. The fact that the science is interdisciplinary further contributes to chaotic communication. Jarcho suggests that a registry be established for this express purpose (1965:1160-1163) and that medical indexing be extended to include paleopathology (1966: 28). Armelagos (1967:2) suggests also that more intensive communication between anthropology and medicine would be highly beneficial to pathology research.

The application of radiographic techniques to diseased bones has yielded interesting and fruitful results. Whereas macroscopic examination reveals only external morphological patterns, X-ray points out interrupted growth patterns, extent and depth of disease involvement, alterations of outer and

inner bone tables, and so forth. McKern and McKern (1969; Sharon McKern 1970) have illustrated practical applications of radiographic information. They point out that whereas arteriosclerosis is frequently thought to be resultant of alcohol, tobacco, modern stresses and a rapid pace of life, X-ray now indicates that arteriosclerosis actually existed long before the appearance of stressful living conditions and the use of alcohol and tobacco. Thus, the factors which appeared to be causally linked were actually only superficially compatible.

The sectioning of bone for microscopic analysis has become a standard technique in paleopathology. Growth alteration and other anomalies appear in section. McLean and Urist (1968) have done innovative work in such bone analysis. Biochemical studies of dried bone, as in distinguishing blood types in prehistoric populations, have been contributed by Boyd (1939), Candela (1939, 1940) and Thieme and Otten (1957). It is currently questionable, however, whether blood typing of prehistoric remains is indeed accurate.

The future of paleopathology is virtually unlimited. As regards research openings, specifically for physical anthropologists, the field is fertile. Current interest in the subject indicates continuing and expanded development of paleopathology.

Primatology

The incorporation of primatology into the framework of physical anthropology is one of the more marked peculiarities of the discipline. For while anthropology, defined as the total study of man, in all his aspects, has broadened its scope in order to shed more light on man's nature and development--as evidenced through the embracing of primatology--yet the study of primates is equally appropriate to another formal discipline, namely mammology. The physical anthropologist, however, approaches primatology from two vantages, the more emphasizing its appropriateness to anthropology. For we have already seen that physical anthropology's unique approach to man is in terms of his bio-social duality. Hence primates--of which man is a member, of course--are likewise approached in terms of the duality. They are investigated as physical organisms, with respect to their morphological structures and functions. And they are investigated as living, behaving animals, possessive of varying degrees of social organization. Finally, the unique anthropological contribution of primatology lies in the integration of the morphological and behavioral aspects of primates. Physical anthropologists have also been able, fortunately, to apply knowledge of the evolution and present character of primates to the evolution and behavior of Homo sapiens.

Primate anatomy may be approached from two viewpoints--that of comparative anatomy and that of anthropometry. And while some duplication of effort probably occurs in this respect, for the most part investigators of primate anatomy and physiology work in an interdisciplinary fashion. Furthermore, the most prolific work in this area has been conducted by Europeans, and therefore is only indirectly relevant to the present study. Foremost among students of primate anatomy is the British W. E. LeGros Clark. His definitive works have synthesized and summarized the history of primates; see, for example, The Antecedents of Man (1959b) and History of the Primates (1959a). And while Clark's theory and interpretations may not always be palatable, especially to those oriented to thought more contemporary than traditional, yet his method and procedure remain impeccable.

American studies in the area of primate anatomy have been, for the most part, individual studies of specific anatomical characters, or of species. Since 1959, the American Journal of Physical Anthropology has published a series of 41 articles on the skin of primates, contributed by a number of researchers. This effort is typical of the American trend toward systematically studying and recording data. Thus, step by step is the corporate body of information assembled.

Perhaps the foremost American student of primate anatomy was Adolph H. Schultz. A native German, educated in that country, he brought his researches to the United States in 1916. From that time he contributed extensively to the growing store of information on primate biology. Spanning the extent of research in primatology, his studies focused on the prenatal growth of primates, including man (1926, 1929); growth and development (1933, 1940, 1942); evolution of the primates; pathology (1939); and individual and specific primate variation. His meticulous and well-documented projects (as, for example, the monograph on age changes and variability in gibbons, 1944) not only contributed to the store of information, but undoubtedly also stimulated further researches.

The study of primate behavior has a rather shorter history than that of anatomy. Irven Devore reports (1965:vii) that African apes were briefly studied as early as 1856. Yet nothing systematic was compiled until Clarence Ray Carpenter began studying howler monkeys in 1931. Devore points out, additionally, that with the exceptions of Carpenter's studies of the howler (1934), gibbon (1940) and rhesus (1942), H. C Bingham's study of the gorilla (1932), and H. W. Nissen's on the chimpanzee (1931), that virtually nothing was accomplished for another 20 years. This brings us, then, within two

decades of the present. A remarkable amount of work has been accomplished during the past two decades, increasing in quantity and quality to the present. Yet it is lamentable that having come so far, we still have so far to go.

During the 1950's the Japanese Monkey Center was established at Kyoto University. During this time also, Stuart A. Altmann undertook to restudy rhesus monkeys on Cayo Santiago. From these beginnings we have reached the present day when, Devore reports, "there are well over 50 individuals from at least nine countries engaged in such studies" (1965:viii). Primary among these efforts are those of the Soviets. It does indeed seem remarkable that so much has been learned about primates in so short a time. Yet it is dismaying that, given the decreasing numbers of undisturbed primates in native environments, only about 50 persons are actively engaged in documenting the behavior of these animals. Not only is physical anthropology engaged in these efforts, but also experimental and comparative psychology, and zoology. Special problems are engendered by interdisciplinary investigation of a topic or research area, as each discipline brings into consideration its own emphasis and conceptual frameworks. Consequently, integrative studies are essential. A case in illustration of cooperative efforts is the Primate Project held during 1962-63 at the Center for Advanced Study of the

Behavioral Sciences, Stanford, California. Devore's Primate Behavior (1965), an indispensable reference work in primatology, is an outgrowth of the project. Primate centers in Atlanta, Georgia, and Davis, California, have fostered similarly cooperative studies.

It is important to note at this point that not all of the subject material considered here is strictly pertinent to a history of American physical anthropology. First, many students other than Americans contribute to the literature, and in the United States alone, disciplines other than physical anthropology are involved. And for the effective study of primates, all of the available literature is essentially important. Second, a number of American scientists carry their investigations to the Old World--a situation which, as we have seen, removes Americans from their native resources and places them in other geographical domains. Even the primates which are found in the Western hemisphere are in Central American and the northern half of South America. Thus there are few native resources, strictly speaking, for the American primatologist.

This deficiency in native primate populations has been met by American physical anthropologists in two specific ways. First, American primatologists function in an international environment. Primatological method and theory is seldom

described as American, Japanese, Russian, Indian or British, for example, but rather is relatively free of nationalistic thought. Perhaps then, a lesson might be learned from this situation which was a result of history and chance. That is, while primatology is a small and new field, it is emerging in a truly international and interdisciplinary atmosphere. We may expect then that as the field grows the science will be able to bypass periods of nationalistic fervor and provincialism.

American physical anthropologists have approached primatology in yet another fashion, however. And once again, the tactic has been typically American in character, although Japanese and Russians have made similar attempts. Americans, having so few primates in the New World, and encountering obstructions--in the form of financial worries, time considerations, geographical distance, etc.--whenever they carry their research enterprises to the Old World, have established their own primate laboratories in the United States. Several primate laboratories, of varying scope, have appeared across the United States, some complete with reconstructed environments closely approximating the native habitats of primates. The primates themselves are imported. These scientists are aware that special problems arise in artifically-constructed environments, especially with respect to what might be termed

normal primate behavior. For primate populations which have been captured, marked, transported and resettled may be expected to endure some forms of trauma and to manifest corresponding behavior. Yet the very awareness of these limitations has enabled primatologists to compensate and account for these factors, and the advantages of having populations of primates at immediate disposal for study far offset the disadvantages. In short, then, American physical anthropologists have been able to function cooperatively in an international and interdisciplinary approach to primatology, while devising their own solutions for the problems encountered.

CHAPTER VII

FUTURE OF THE DISCIPLINE

The natural evolution of American physical anthropology has witnessed a progression from an originally unified system, wherein certain common interests were examined and common goals were accomplished through similar methodologies, to a highly diversified modern science. In recent decades, as the discipline has grown and matured, more scholars from highly diversified backgrounds have united under the banner of this discipline and have come to ask a multitude of questions, answerable only through different procedures and methodologies. Coincident with diversification and specialization of scientific pursuit has been a general disunification of personnel. It is to be expected, moreover, as the scope of research expands, that scientists will become farther separated from one another theoretically and practically, and that they will be bound increasingly by only highly specialized research interests. The specialist in human paleopathology, for example, encounters only occasionally the researches of the behavioral primatologist, and the genetical anthropologist occupies his time far differently than the human identification expert.

The extreme diversification within the American psychological enterprise may perhaps give us insight, by analogy, into the possible future nature of anthropological specialization and disciplinary fragmentation.

That physical anthropologists are obviously less united than at earlier periods is hardly cause for alarm. Rather, it is a logical and expected manifestation of growth and development. The present is an auspicious time, however, for summarization and integration. The "armchair anthropologist" has in the past fallen into a measure of disrespect, occasionally subjected to caustic satire. His day is returning, however, as the individual physical anthropologist encounters more and more data which his training has hardly prepared him to interpret adequately, but the incorporation of which is demanded in an overall anthropological interpretation.

The need for anthropological integration, in other words, is more pronounced now than ever before. The bio-social nature of man, we are learning, is far more complex than once supposed. Physical anthropologists, through undertaking to study this peculiar duality of man, have placed themselves at a particularly advantageous position for expounding on the real nature of man, viewed as a multi-faceted entity. On the American front there have been few such syntheses of the broader findings of physical anthropology. Ashley Montagu

has made significant contributions in this respect during the past two or three decades. In particular, The Human Revolution (1967) expresses his theoretical views on the implications of where man has been, where he is and why, and what directions his future may assume.

It should be abundantly clear, by this time, that future prospects in American physical anthropology are encouraging. In a sense, we have only begun. Only now, after a century of thinking, wondering, questioning, searching, are we beginning to catch a glimmer of what is man, the bio-social animal--of what has transpired since the origin of the hominids, or even the emergence of the order Primate. Only now can we begin to fathom the void beyond simple observation and description. We are just beginning to understand the dynamics of life and evolution--the breathing flesh and blood which we ourselves are. It is something of an understatement, then, to characterize the future of physical anthropology as exciting.

The foregoing discussions--especially those relating to theory, physical data, and original research--have given at least some indication of what may be expected in the future. To be sure, the future is but a subtle continuation of the present, as changes both minor and pronounced are manifested. Likewise, the discipline as we know it today, and function

within its frameworks, is an evolutionary product of the past century and before. But slow as the process may be, a sort of metamorphosis has taken place. The particular metamorphic stage which we are currently experiencing--a process so subtle that an occasional scientist can see fit to deny and denounce its working presence--is that of a gradual reorientation toward the techniques of genetics and molecular biology. The change demands, in its wake, that function and process be addressed and studied until they are understood. The demand intensifies, the frontiers are boundless. Today's orientation prescribes that dynamics must be understood and integrated into the scheme of anthropology.

Certain areas of investigation are more susceptible or conducive to pronounced change than are others. Among them is paleopathology. In its relatively youthful state, the study of paleopathology awaits the development of new techniques, even the formulation of new questions. For the science of early man's diseases and body traumas has barely been founded. Research to the present day has been for the most part descriptive. Further, the scarcity of remains available to the paleopathologist makes difficult a dynamic approach. And after exerting extreme effort simply to discern and accurately describe pathological manifestation in the human skeleton, he finds it regrettable that there remains so little

that can safely be said regarding cause and clinical manifestations in the living person. A recent study by the author and T. D. Stewart (1969) illustrates this problem in some detail. Lesions of the frontal bone were described and analyzed in several American Indian populations. While the lesions are reminiscent of syphilis and yaws manifestations, it can hardly be said at present that these lesions were the result of any specific disease. The logical step, however, is to project a new realm of comparative study, wherein modern medical samples would be compared with archaeological samples through microscopic and structural-constitutional analysis. At present, however, this type of study is pursued by only a few paleopathologists. And the techniques and results are far from being general anthropological knowledge.

Environmental adaptation is an even newer branch of investigation for physical anthropologists, which should yield increasing returns in the future. And again, the task for the immediate future is to begin to discern what genetic mechanisms have assisted in the production and maintenance of adaptive features. How does environmental adaptation come about? What, specifically and functionally, does environmental adaptation mean? Why do environmental adaptations survive when the harsh conditions which created them are no longer a crucial threat? These questions, when answered, will draw us somewhat

nearer to the integrated functional picture which is obscured for us at present. Moreover, such lines of investigation will inevitably lead the physical anthropologist into association with other scientists of other disciplines, where he will participate in the interdisciplinary endeavors which are so characteristic of the present period.

Human engineering is a promising field for future development, and it is in this area that the physical anthropologist as academician can offer immediate relevance of his tools and findings to the outside world. It has been amply demonstrated in the past that physical anthropology can have numerous practical applications in the worlds of business and manufacturing. Hence we can expect that as the industrial world mushrooms and the academic world becomes more sophisticated, more and more channels should be opened for mutual exchange. More accommodating design of products where human utilization and consumption are concerned, should be easily attained through proper usage of anthropological knowledge. If the space program and its attendant demands continue to proliferate, more and more demands will be placed on the anthropologist for anthropometric as well as functional knowledge. To meet this need, however, physical anthropologists should be consciously charting the future.

And as for genetic and other biologically-oriented studies which have recently arrived at the forefront of the physical anthropology enterprise, we can predict with all due certainty that the scope and output will continue to increase. Here again the physical anthropologist must be prepared to interact on a broad interdisciplinary level, a prospect which necessitates not only the ability to communicate the anthropological point of view, but also to grasp and understand a perspective which may feel uncomfortably foreign at the outset. Whether the discipline can continue to meet the rapidly expanding demands, and maintain direction for maximizing the future, is the test of the sixth prerequisite. The future of physical anthropology is largely dependent upon the attitudes and insight of investigators. The challenge awaits the motivated scientist.

While a traditional orientation toward the future prescribes that we seek newer and better research methodology and techniques, a more exciting prospect may lie in applying the findings of physical anthropology to the needs of modern society. We have seen how in the past the technique of human skeletal identification has drawn from the resources of the U.S. Graves Registration and has in turn contributed to the laboratory processing of war dead. The research skills and

data on identification obtained thereby have subsequently enabled physical anthropologists to render services to law enforcement agencies. It is logical to expect that in the near future new and improved means will be obtained for correctly identifying unknown human remains. Microscopic and chemical analysis of bone should yield increasing accuracy, and further understanding of the significance and validity of racial characteristics should contribute likewise.

The Statements on Race prepared for UNESCO provided another avenue for anthropologists to communicate with the world in a practical fashion. In the case of the UNESCO Statements, however, the potential was not fully realized. The implications of race constitute one of the major social problems facing the world today. Everyone speaks of race--the politician and the poet, the housewife and the revolutionary, bankers, comedians and businessmen. But who can really understand the meaning of the concept "race"? Race, an essentially biological concept, has become a predominant social issue, if only because people have made it so. The trend may be regrettable in some respects. But since it seems to be fact, we can best take the matter from here, and proceed toward righting the wrongs. In this light, it is sad to observe that the few people who might well have the wherewithal to rectify some of the greatest injustices in the world today

are not exercising their powers in especially meaningful ways. There are exceptions, but these are all too rare.

The physical anthropologist is among the handful of people who are in a position to shed scientific light on the darkness of race. It is time, therefore, for the physical anthropologist to go into the world, meet the layman, and somehow attempt to tell him what race is. The anthropologist should be able, through his biological understanding of the nature of race, to assist the layman in incorporating accurate and healthy racial attitudes into his everyday life. Inevitably, a meeting with the non-academic world will push the scientist into the realm of politics. And to most of us this is a lamentable prospect. Perhaps, however, political confrontation can be avoided, if science can be held above partisan issues in the presentation of truth as the scientist discovers it.

Thus it is perhaps the humanistic duty of physical anthropologists, biologists and geneticists to approach this task. It is a chore which hardly will be palatable to most physical anthropologists who, as academicians, may prefer to continue their scientific pursuits unhampered by social demands. On the other hand, it is an all too human disposition to pass responsibility to another person or group with vested interests, and the problems of the world can hardly be ignored any longer.

Daily it appears increasingly true that our world is in dire straits. We have created situations--blindly, unknowingly, unthinkingly--which now threaten to destroy us. Racial prejudice and discrimination, hauntingly real spectres of destruction and inhuman injustice, threaten the goals, accomplishments and rights of mankind. And there are countless other problems.

Environmental destruction is resulting in the pollution of our human bodies and indirectly causes greater mental and emotional anguish, thereby lessening man's capacity for con-- sidering the welfare of his fellow man. Environmental crisis has placed man on a self-defensive battlefield, where resources cannot meet population demands. The results are becoming painfully obvious. And on another front governmental institutions the world over are suffering disrespect, violent criticism and physical attack, often justifiably, from a disturbed and confused mass of citizenry. Revolution threatens many nations, and the most powerful and stable unions of the world have not escaped this dilemma. Revolution, a drastic and usually violent route to change, derives from and wrenches to the core social organization and cultural mores.

Behind all of these socially disruptive forces is a profound lack of understanding on the part of people involved. And the modern, progressive, industrial trend in which we currently find ourselves has man plotted against problems of

such magnitude that he usually feels stymied, if not impotent. Then, to his dismay, he finds that passing the buck becomes less effective as it becomes more common.

In short, the time for action is Now. The opportunities and products, the knowledge and wisdom which the world has given to physical anthropology, as well as all other physical, natural and social sciences, has provided the avenues to our present condition. Further, the United States has provided one of the finest scientific atmospheres--in terms of encouragement, license, funding, fostering and sponsoring--that this planet has yet witnessed. We have reached an apex of scientific and social knowledge, a continually rising apex. Yet our Earth needs help, and on some fronts we are rapidly losing critical battles. It would seem that it is time for science to repay an outstanding debt. The products and knowledge which have been reaped from the Earth must now be returned in order to rescue the mother planet.

Anthropological science can wage this battle in a way no other social institution can: with its understanding of human prehistory, evolution and development, of individual and population variation, of the physiological requirements of life. History has shown, however, and present events continue to validate, that problems are rarely solved by mass attack--by razing slums and creating high-rise slums, by

enacting sweeping legislation, by marching, rioting and picketing, or by instituting research foundations. For while the outer manifestations of social problems are frequently altered, and often toward improvement, the problem remains in disguised form until its core has been exposed and altered. The basic problem, in other words, lies in the human mind. And anthropologists are currently armed with sufficient information to begin combatting these problems. A white man does not relate to the needs of a black man until he understands the past and present contexts of that man. A black man does not understand a white man's racial hostilities until he begins to fathom the subtle social conditioning which has produced those attitudes. Behind each interpersonal disaster is the failure to understand the very nature of another man. Likewise, man continues to rape his world--polluting, destroying and draining it--because he fails to grasp the ultimate significance, in terms of individual human destruction, of his actions.

This is a grim picture indeed. But it need not remain so. For we have the future, and we trust that it can be directed. Mankind can, hopefully, be saved from the slough of his own blind devising. Despite the intricacies, the difficulties and the pitfalls, the way is startingly clear: Each man must enlighten himself, must know that Understanding

is the key, and thus must Understand. The task will not be accomplished through the efforts of a single man. But rather, as each man faces his reality and accounts for himself, the composite effort which will result is the avenue of regeneration which can provide a harmonious future.

It is here again that physical anthropologists may be of service. The anthropologist can demonstrate how, when, where and why man came to be as he is--so like, yet so unlike every other man. He can show the reasons for and the character of present needs. Finally, we begin to see that physical anthropology, after a century of growth and development, is only beginning to mature. The real problems await its attention. We physical anthropologists are people. Let us strive to better the lot of one another.

LITERATURE CITED

Allen, H.
1890 The Clinical Study of the Skull. Smithsonian Inst. Toner Lectures. Washington.

1896 Crania from the Mounds of the St. John's River.

1898 Study of Hawaiian Skulls.

Ackerknecht, E. H.
1962 Paleopathology. In Anthropology Today: Selections, S. Tax, ed., pp. 39-45. Univ. of Chicago Press, Chicago.

Armelagos, G. J.
1967 Future work in paleopathology. Mus. Northern Ariz., Tech. Ser. No. 7, Misc. Papers in Paleopathology: I, pp. 1-8.

Bass, W. M.
1963 The use of heavy power equipment in the excavation of human skeletal material. Abstract. Plains Anthrop. 8:122-123.

1964a The Variation in Physical Types of the Prehistoric Plains Indians. Plains Anthrop., Mem. 1. Weber Bulletin Service, Lincoln, Nebr.

1964b The Physical Anthropology of the Sully Site, 39SL4. Unpublished manuscript on file at the Midwest Regional Office, National Park Service, Omaha, Nebr.

Bass, W. M., D. R. Evans and R. L. Jantz
1970 Physical Anthropology and Archaeology of the Leavenworth Site Cemetery 39CO9, Carson County, S. Dak. Univ. of Kansas Publ. in Anthrop. No. 2. Lawrence.

Beddoe, J.
1887-88 On the stature of the older races of England, as estimated from the long bones. J. Royal Anthrop. Inst. 17:202-207.

Berger, P. L.
1963 Invitation to Sociology: a humanistic perspective. Anchor Books, Garden City, New York.

Bingham, H. C.
1932 Gorillas in a native habitat. Carnegie Inst. Wash. Publ. 426:1-66.

Birdsell, J. B.
1951 Review: Genetics and the Races of Man, by W. C. Boyd. Am. J. Phys. Anthrop., n.s. 9:219-223.

1952 Brief communication: On various levels of objectivity in genetical anthropology. Am. J. Phys. Anthrop., n.s. 10:355-362.

Boas, F.
1940 Race, Language and Culture. Collier-Macmillan, Ltd., London.

Boucher, B. J.
1955 Sex differences in the foetal sciatic notch. J. Forensic Med. 2:51-54.

1957 Sex differences in the foetal pelvis. Am. J. Phys. Anthrop., n.s. 15:581-600.

Boyd, L. G. and W. C. Boyd
1939 Blood group reactions of preserved bone and muscle. Am. J. Phys. Anthrop. 25:421-434.

Boyd, W. C.
1950 Genetics and the Races of Man. D. C. Heath and Co., Boston.

Brew, J. O.
1968 One Hundred Years of Anthropology. Harvard Univ. Press, Cambridge.

Brooks, S. T.
1955 Skeletal age at death: reliability of cranial and pubic age indicators. Am. J. Phys. Anthrop., n.s. 13:567-597.

Bryan, A. L.
1969 Early man in America and the Late Pleistocene chronology of Western Canada and Alaska. Current Anthrop. 10:339-365.

Buettner-Janusch, J.
1966 Origins of Man. John Wiley and Sons, Inc., New York.

Buettner-Janusch, J.
1969 The nature and future of physical anthropology. Trans. New York Acad. Sci., ser. II, vol. 31, no. 2, pp. 128-138.

Candela, P. B
1939 Blood-group determinations upon the bones of thirty Aleutian mummies. Am. J. Phys. Anthrop. 24:361-383.

1940 Reliability of blood tests on bones. Am. J. Phys. Anthrop. 27:367-381.

Carpenter, C. R.
1934 A field study of the behavior and social relations of howling monkeys. Comp. Psychol. Monogr. 10(48):1-168.

1940 A field study in Siam of the behavior and social relations of the gibbon, Hylobates lar. Comp. Psychol. Monogr. 16(5):1-212.

1942 Sexual behavior of free ranging rhesus monkeys, Macaca mulatta. J. Comp. Psychol. 33:133-142.

Clark, W. E. LeGros
1959a The Antecedents of Man; an introduction to the evolution of the primates. Harper Torchbooks, New York.

1959b History of the Primates. Phoenix Books, Univ. of Chicago Press, Chicago.

Comas, J.
1960 Manual of Physical Anthropology. Charles C. Thomas, Springfield, Ill.

Creed, Ed.
1930 Boston Society of Natural History, Centennial Volume. Boston.

Devore, I.
1965 Primate Behavior. Holt, Rinehart and Winston, New York.

Dupertuis, C. W. and J. A. Hadden, Jr.
1951 On the reconstruction of stature from long bones. Am. J. Phys. Anthrop., n.s. 9:15-54.

Dwight, T
1890 The closure of the sutures as a sign of age. Boston Med. and Surg. J. 122:389-392.

Genoves, S. y A. Romano
1958 Algunos aspectos relacionados con los restos de Tepexpan. V. International Congress of Prehistoric and Protohistoric Sciences, Hamburg.

Goldstein, M.
1963 Human paleopathology. J. Nat. Med. Assn 55:100-106.

Greulich, W. W.
1951 The growth and development of Guamanian school children. Am. J. Phys. Anthrop., n.s. 9:55-70.

1958 Growth of children of the same race under different environmental conditions. Science 127:515-516.

Greulich, W. W. and S. I. Pyle
1959 Radiographic Atlas of Skeletal Development of the Hand and Wrist. Stanford Univ. Press.

Heizer, R. F. and S. F. Cook
1952 Fluorine and other chemical tests of some North American human and fossil bones. Am. J. Phys. Anthrop. 10:299-300.

Hooton, E. A
1918 On certain Eskimoid characters in Icelandic skulls. Am. J. Phys. Anthrop. 1:53-76.

1930 The Indians of Pecos Pueblo. Yale Univ. Press, New Haven.

1932 Up From the Ape. Macmillan Co., New York.

1937 Apes, Men and Morons. G. P. Putnam's Sons, New York.

1939a Crime and the Man. Harvard Univ. Press, Cambridge.

1939b The American Criminal. An anthropological study. Harvard Univ. Press, Cambridge.

Howells, W. W.
1943 Physical anthropology as a technique. Am. J. Phys. Anthrop., n.s. 1:355-361.

Hrdlicka, Ales
1912 Early Man in South America. Bur. Amer. Ethnol. Bull. 52. Washington.

Hrdlicka, Ales
1918 Recent Discoveries Attributed to Early Man in America. Bur. Amer. Ethnol. Bull. 66. Washington.

1919a Anthropometry. D. Skeletal Parts: The Skull. Amer. J. Phys. Anthrop. 2:401-428.

1919b Physical anthropology. Its scope and aims; its history and present status in the United States. Wistar Inst. of Anat. and Biol., Philadelphia.

1939 Practical anthropometry. Wistar Institute, Philadelphia.

Hulse, F. S.
1963 The Human Species. An introduction to physical anthropology. Random House, New York.

Hunt, E. E., Jr., and I. Gleser
1955 The estimation of age and sex of preadolescent children from bones and teeth. Am. J. Phys. Anthrop., n.s. 13:479-487.

Jaffe, H. L.
1966 Discussion of W. G. J. Putschar's "Problems in the pathology and palaeopathology of bone." In Human Paleopathology, S. Jarcho, ed., pp. 65-68. Yale Univ. Press, New Haven.

Jantz, R. L.
1970 Change and Variation in Skeletal Populations of Arikara Indians. Unpublished doctoral dissertation. Univ. of Kansas, Lawrence.

Jarcho, S.
1965 Human paleopathology, the development and present condition in the United States. Am. Med. Assn. Arch. Path. 79:425-427.

1966 The development and present condition of human palaeopathology in the United States. In Human Paleopathology, S. Jarcho, ed., pp. 3-29. Yale Univ. Press, New Haven.

Jenks, A. E.
1936 Pleistocene Man in Minnesota. A fossil homo sapiens.

1937 Minnesota's Browns Valley Man and associated burial artifacts. Am. Anthrop. Assn. Mem. 49.

Jewel, John
1963 An Apology of the Church of England. Edited by J. E. Booty. Cornell Univ. Press, Ithaca, New York. (First published 1564)

Kaplan, B.
1951 Report of the Sixth Annual Summer Seminar in Physical Anthropology. Wenner-Gren Foundation, New York.

Kardiner, A. and E. Preble
1961 They Studied Man. Mentor Books, New York.

Kerley, E. R. and W M. Bass
1967 Paleopathology: meeting ground for many disciplines. Science 157:638-644.

Klebs, A. C.
1917 Palaeopathology. Johns Hopkins Hosp. Bull. 28:261-266.

Krause, R. A.
1969 The Leavenworth Settlement. Unpublished manuscript on file at the Midwest Regional Office, National Park Service, Omaha, Nebr.

Krogman, W. M.
1962 The Human Skeleton in Forensic Medicine. Charles C. Thomas, Springfield, Ill.

Lasker, G.
1964 The "new" physical anthropology seen in retrospect and prospect. The Centennial Review of Arts and Sciences, Michigan State Univ., pp. 348-366.

Lombroso, C.
1887 L'homme criminel, criminel-né--fou moral--épileptique. F. Alcan, Paris.

1891 L'anthropologie criminelle et ses récents progrès. Germer-Baillière, Paris.

1892 Nouvelles recherches de psychiatrie et d'anthropologie criminelle. Germer-Bailliere, Paris.

1895 Die Anarchisten. Hamburg.

1911 Crime, its causes and remedies. Trans. by Henry P. Horton. Little, Brown and Co., Boston.

Lombroso, C. and W. Ferrero
1895 The Female Offender. T. F. Unwin, London.

Lowie, R. H.
1937 The History of Ethnological Theory. Holt, Rinehart and Winston, Inc., New York.

Lyon, W. S.
1970 A Multivariate Statistical Analysis of Arikara Crania from the Northern Plains. Unpublished doctoral dissertation. Univ. of Kansas. Lawrence.

Manouvrier, L.
1892 Determination de la taille d'apres les grands os des membres. Rev. Mem. de l'Ecole d'Anth. 2:227-233; Mem. Soc. d'Anth. 2e ser. 4:347-402, 1893.

McKern, T. W.
1957 Estimation of skeletal age from combined maturational activity. Am. J. Phys. Anthrop., n.s. 15:399-408.

McKern, T. W. and S. S. McKern
1969a Human Origins. Prentice-Hall, Inc., Englewood Cliffs, New Jersey.

1969b Prehistoric disease and primitive medicine. Mankind 2:78-87.

1969c The secrets that dead men's bones tell. Science Digest 66:30-34.

McKern, T. W. and T. D. Stewart
1957 Skeletal age changes in young American males, analyzed from the standpoint of identification. Headqu. QM Res. and Dev. Command., Tech. Rep. EP-45, Natick, Mass.

McKern, S. S.
1970 Radiography: New tool for retrieving the wealth of the Pharaohs. Science Digest 68(1):8-13.

McKern, S. S. and T. W. McKern
1970 Odyssey: The peopling of the New World. Mankind 2(6):58-69.

McLean, F. C. and M. R. Urist
1968 Bone. Fundamentals of the physiology of skeletal tissue. Univ. of Chicago Press, Chicago.

Meigs, J. A.
1861 The mensuration of the human skull. North-Amer. Med. Chirurg. Review. September, pp. 837-861.

Merton, R. and M. F A. Montagu
1940 Crime and the Anthropologist. Am. Anthrop. 42: 384-408.

Miller, G. S.
1918 The Piltdown jaw. Am. J. Phys. Anthrop. 1:25-52.

Montagu, M. F. A.
1938 Ageing of the skull. Am. J. Phys. Anthrop. 23:355-375.

1941 Physical anthropology and anatomy. Am. J. Phys. Anthrop. 28:261-271.

1944 Ales Hrdlicka, 1869-1943. Amer. Anthrop. 46:113-117.

1967 The Human Revolution. Bantam Books, New York.

Moodie, R.
1923 Paleopathology: An introduction to the study of the ancient evidences of disease. Univ. of Ill. Press, Urbana.

Morton, S. G.
1834 Synopsis of the Organic Remains of the Cretaceous Group of the United States. Key and Biddle, Philadelphia.

1839 Crania Americana; or, a comparative view of the skulls of various aboriginal nations of North and South America. Dobson, Philadelphia.

1842 Introductory lecture on: "The diversities of the human species," delivered before the Medical Class of Pennsylvania College in November 1842. Reported in Types of Mankind, by J. C. Nott and G. R. Gliddon.

1848 Account of a craniological collection. Trans. Amer. Ethnol. Soc., New York. II:217-218.

Newman, M T.
1951 Review of Races... by Coon, Garn and Girdsell. Boletin Biblio. de Anthrop. Amer. 8(2):188-192.

1953 The application of ecological rules to the racial anthropology of the aboriginal New Yorld. Amer. Anthrop. 55:311-327.

Nissen, H. W.
1931 A field study of the Chimpanzee. Comp. Psychol. Monogr. 8(1):1-122.

Nolan, E. J.
1913 Introduction. In An Index to the Scientific Contents of the Jour. and Proc. of the Acad. of Nat. Sci. of Philadelphia 1812-1912. Acad. of Nat. Sci., Philadelphia.

Nott, J. C. and G. R. Gliddon
1854 Types of mankind: or, Ethnological researches based upon the ancient monuments, paintings, sculptures, and crania of races, and upon their natural, geographical, philological and biblical history: illustrated by selections from the inedited papers of Samuel George Morton. Lippincott, Grambo and Co., Philadelphia.

Packard, F. R.
1942 Medicine and the American Philosophical Society. In The Early History of Science and Learning in America, pp. 91-102. Amer. Phil. Soc., Philadelphia.

Parsons, F. G. and C. R. Box
1905 The relation of the cranial sutures to age. J. Royal Anthrop. Inst. 35:30-38.

Patterson, H. S.
1854 Memoir of the life and scientific labors of Samuel George Morton. In Types of Mankind, by J. C. Nott and G. R. Gliddon, pp. xvii-lvii.

Pearson, K.
1899 Mathematical contributions to the theory of evolution. V. On the reconstruction of the stature of prehistoric races. Phil. Trans. Royal Soc. London 192: 169-244.

1914-15 On the problem of sexing osteometric material. Biom. 10:479-487.

Putschar, W. G. J.
1966 Problems in the pathology and palaeopathology of bone. In Human Paleopathology, S. Jarcho, ed., pp. 57-65.

Pyle, S. I. and N. L. Hoerr
1955 Radiographic Atlas of Skeletal Development of the Knee. Charles C. Thomas, Springfield, Ill.

Pyle, S. I. and L. W. Sontag
1943 Variability in onset of ossification in epiphyses and short bones of the extremities. Am. J. Roentgenol. 49:795-798.

Reynolds, E. L.
1945 The bony pelvic girdle in early infancy. A roentgenometric study. Am. J. Phys. Anthrop., n.s. 3:321-354.

1947 The bony pelvis in prepuberal childhood. Am. J. Phys. Anthrop., n.s. 5:165-200.

Schultz, A. H.
1926 Fetal growth in man and other primates. Quart. Rev. of Biol. 1:465-521.

1929 The technique of measuring the outer body of fetuses and of primates in general. Contr. to Embr., vol. 20. Carnegie Inst. Wash. Publ. 394, pp. 213-257.

1933 Observations on the growth, classification and evolutionary specialization of gibbons and siamangs. Human Biol. 5:212-255; 385-428.

1939 Notes on diseases and healed fractures of wild apes and their bearing on the antiquity of pathological conditions in man. Bull. Hist. of Med. 7:571-582.

1940 Growth and development of the chimpanzee. Contr. to Embr., vol. 28. Carnegie Inst. Wash. Publ. 518, pp. 1-63.

1942 Growth and development of the proboscis monkey. Bull. Mus. Comp. Zool. 89:277-314.

1944 Age changes and variability in gibbons. Am. J. Phys. Anthrop., n.s. 2:1-129.

Sellards, E. H.
1916 Human remains and associated fossils from the Pleistocene of Florida. Florida Geol. Survey, 8th Ann. Rept., pp. 123-160.

Sergi, S.
1947 Terminologia e divisione delle sienze dell'uomo. I. Resultati di un'inchiesta internazionale. Rivista di Antropologia 35:5-83.

Singer, R.
1953 Estimation of age from cranial suture closure. J. Forensic Med. 1:52-59.

Smith, G. E. and W. R. Dawson
1924 Egyptian mummies. George Allen and Unwin, Ltd., London.

Steele, D. G.
1970 The Calcaneus and Talus: Discriminant functions for estimation of sex among American whites and Negroes. Unpublished doctoral dissertation. Univ. of Kansas. Lawrence.

Stewart, T. D.
1946 A reexamination of the fossil human skeletal remains from Melbourne, Florida, with further data on the Vero skull. Smithsonian Misc. Coll. vol. 106, no. 10.

1947 Hrdlicka's Practical Anthropometry. Wistar Inst. of Anat. and Biol, Philadelphia. (Fourth ed. in 1952)

1951a Brief communication: Objectivity in race classification. Am. J. Phys. Anthrop., n.s. 9:470-472.

1951b Scientific responsibility (Editorial). Am. J. Phys. Anthrop. 9:1-3.

1951c Three in one: Physical anthropology, genetics, statistics. J. Heredity XLII:255-256, 260.

1960 A physical anthropologist's view of the peopling of the New World. Southwestern Jour. of Anthrop. 16:259-273.

Stewart, T. D. and L. G. Quade
1969 Lesions of the frontal bone in American Indians. Amer. J. Phys. Anthrop. 30:89-110.

Strandskov, H. H. and S. L. Washburn
1951 Editorial: Genetics and physical anthropology. Am. J. Phys. Anthrop., n.s. 9:261-263.

Telkka, A.
1950 On the prediction of human stature from the long bones. Acta Anat. 9:103-117.

de Terra, H., J. Romero, T. D. Stewart and others
1949 Tepexpan Man. Viking Fund Publ. in Anthrop. no. 11. New York.

Thieme, F. P. and C. M. Otten
1957 The unreliability of blood typing aged bone. Am. J. Phys. Anthrop., n.s. 15:387-397.

Thieme, F. P. and W. J. Schull
1957 Sex determination from the skeleton. Human Biol. 29:242-273.

Todd, T. W
1920 Age changes in the pubic bone: I. The male white pubis. Am. J. Phys. Anthrop. 3:285-334.

1921a Age changes in the pubic bone: II. The pubis of the male Negro-white hybrid; III. The pubis of the white female; IV. The pubis of the female Negro-white hybrid. Am. J. Phys. Anthrop. 4:1-70.

1921b Age changes in the pubic bone: V. Mammalian pubic metamorphosis. Am. J. Phys. Anthrop. 4:333-406.

1921c Age changes in the pubic bone: VI. The interpretation of variations in the symphysial area. Am. J. Phys. Anthrop. 4:407-424.

1923 Age changes in the pubic symphysis: VII. The anthropoid strain in human pubic symphyses of the third decade. J. Anat. 57:274-294.

1930 Age changes in the pubic bone: VIII. Roentgenographic differentiation. Am. J. Phys. Anthrop. 14:255-271.

1931 Differential skeletal maturation in relation to sex, race, variability and disease. Child Dev. 2:49-65.

1933 Growth and development of the skeleton. In Growth and Development of the Child, Pt. II: Anatomy and Physiology, pp. 26-130. Century, New York.

Trotter, M. and G. C. Gleser
1951a The effect of ageing on stature. Am. J. Phys. Anthrop., n.s. 9:103-117.

1951b Trends in stature of American whites and Negroes born between 1840 and 1924. Am. J. Phys. Anthrop., n.s. 9:427-440.

1952 Estimation of stature from long bones of American whites and Negroes. Am. J. Phys. Anthrop., n.s. 10:473-514.

1958 A re-evaluation of estimation of stature based on measurements of stature taken during life and of long bones after death. Am. J. Phys. Anthrop., n.s. 16:79-123.

UNESCO
1951 Statements on Race. Am. Anthrop. 53:142-145.

1952 Statement on Race. Am. J. Phys. Anthrop. 10:363-368.

Virchow, R.
1862 Die Einheitsbestrebungen in der wissenschaftlichen Medicin, 2d ed., Hamm.

1896 Beitrag zur Geschichte der Lues. Dermat Ztschr. 3:4.

Walsh, W. H.
1960 Philosophy of History: An introduction. Harper Torchbooks, New York.

Warren, J. C.
1822 A Comparative View of the Sensorial and Nervous System in Men and Animals. Ingraham, Boston.

Washburn, S. L. and S. R. Detwiler
1943 An experiment bearing on the problems of physical anthropology. Am. J. Phys. Anthrop., n.s. 1:171-190.

Washburn, S. L.
1948 Sex differences in the pubic bone. Am. J. Phys. Anthrop., n.s. 6:199-208.

1949 Sex differences in the pubic bone of Bantu and Bushman. Am. J. Phys. Anthrop., n.s. 7:425-432.

Washburn, S. L.
1951 The new physical anthropology. Trans. New York Acad. Sci., ser. II, vol. 13, pp. 298-304.

1953 The strategy of physical anthropology. In Anthropology Today, S. Tax, ed. Wenner-Gren Foundation, New York.

1968 One hundred years of biological anthropology. In One Hundred Years of Anthropology, J. O. Brew, ed. Harvard Univ. Press, Cambridge.

Wells, L. H.
1959 Estimation of stature from longbones: a reassessment. J Forensic Med. 6:171-177.

Wilder, H. H.
1920 A laboratory manual of anthropometry. P. Blackiston's Son and Co., Philadelphia.

Wyman, J.
1843-44 Observations on the external characters, habits, and organization of the Troglodytes niger, Geof. Boston J. Nat. Hist. IV:362-376, 377-386.

1845-47 Notice of the external characters, habits and osteology of Troglodytes gorilla, a new species of ourang from the Gaboon river. Boston J. Nat. Hist. V:417-422; Ann. Sci. Nat., 1851, XVI:176-182; Proc. Boston Nat. Hist. Soc., 1845-48, II:245-248; Am. J. Sci., 1849, VIII:141-142.

1848 A new species of Troglodytes. Silliman's J. V:106-107.

1849 A description of two additional crania of the enge-ena (Troglodytes gorilla, Savage and Wyman) from Gaboon, Africa. Proc. Boston Soc. Nat. Hist. III:179; Amer. J. Sci., 1850, IX:34-45; New Phil. J. Edinb., 1850, XLVIII:273-286.

1854-56 Dissection of a black Chimpanzee (Troglodytes niger). Proc. Boston Soc. Nat. Hist. V:274-275.

1860 Account of the collection of gorillas made by Mr. Du Chaillu. Proc. Boston Soc. Nat. Hist., Jan. 4, 1860.

Wyman, J.
1861 On bones of a gorilla recently obtained in western equatorial Africa. Proc. Boston Soc. Nat. Hist., Oct. 2, 1861.

1863 Observations on the cranium of a young gorilla. Proc. Boston Soc. Nat. Hist. IV:203-206.

1864 On malformations. Proc. Boston Soc. Nat. Hist., Oct. 19, 1864.

1866 On the distorted skull of a child from the Hawaiian Islands. Proc. Boston Soc. Nat. Hist., Oct. 17, 1866.

1868 On symmetry and homology in limbs. Proc. Boston Soc. Nat. Hist. XI:246-278.